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A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

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VOL. XXI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, John Gough, Esq.; Dr. Thomas Stewart Traill; Opsimath; A. Dilethante; Mr. William Cooke; Mr. William Skrimshire, Jun.; Mr. Robert Bancks; P. Barlow, Esq.; A Correspondent; Dr. Beddoes; J. B.; John Bostock, M. D.; W. Saint, Esq,; Mr. A. Combes; J. A. De Luc, Esq.; William Walker, Esq.; W. Moore, Esq.; James Woodhouse; Mr. B. Cook; Mr. J. Acton; S. Vince.

Of Foreign Works, M. Vauquelin; John Michael Haussmann; M. Gueniveau; M. Berthier; M. V. Auarie; M. Frederic Mohs; M. Tonnelier; M. P. Turpin; M. Theodore de Saussure; A. Avogadro; J. C. Delamatherie; Lewis Cordier; J. P. D'Aubuisson; Professor Proust; R. J. Hauy.

And of British Memoirs abridged or extracted, Thomas Andrew Knight, Esq.; Lord Ribblesdale; Thomas Thomson, M. D. F. R. S.; Mr. William Hardy; Mr. Henry Ward; Mr. Martin Furniss; Abraham Parsons, Esq.; Mrs. Hooker; Mr. William Murdoch; Everard Home, Esq. F. R. S.; Mr. Gilbert Gilpin; Mr. Christopher Wilson; J. Witley Boswell, Esq.; Major Spencer Cochrane; William Hyde Wollaston, M. D. Sec. R. S.; Mr. George Smart; Mr. Joseph Davis; Mr. S. Mendham; Mr. Edward Massey; Edmund Turrell; Robert Buchanan; Mr. Joseph Collier; Mr. William Shipley; Humphry Davy, Esq. Sec. R. S. M. R. I. A.

The Engravings consist of 1. Dr. Traill's Mercurial exhausting Machine; 2. Problem by J. Gough, Esq.; 3. Mr. Hardy's Correction of Vibration in Time Keepers; 4. Mr. Henry Ward's Compensation Pendulum; 5. Mr. Furniss's Air-tight Door Hinge; 6. Mr. C. Gilpin's Machine for raising Coals; 7. Mr. C. Wilson's secure Sailing or Life Boat; 8. Mr. J. Boswell's improved Capstan; 9. Mr Mendham's Escapement; 10. Mr. Davis's Chimney Brush; 11. Mr. Davis's Pannels for Security; 12. M. Tonnelier on the Meïonite; 13. Mr. Massey's Patent Log; 14. Mr. Massey's Sounding Machine; 15. Diagrams illustrating the Problem respecting the Radius of Curvature; 16. Instruments for the Construction of improved Chemical Muffles; 17. Mr. Collier's Ship's Stove; 18. Mr. Shipley's Floating Light; 19. Mr. Acton's Improvement in the Still; 20. Crystals of Carbonate of Lime.

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JOURNAL

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NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1808.

ARTICLE I.

A Mathematical Problem: by John Gough, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, August* 5, 1808.

THE insertion of the following problem, with the investigation of it, in your valuable Journal, will oblige

Yours, &c.
JOHN GOUGH.

The problem proposed.

Problem. To a given arc of a circle a, let it be required to add another z, making the sum of the two arcs equal to the tangent of the latter, t, viz. $a+z=t$.

Contains nothing absurd.

(A) We may show in the following manner, that the problem contains nothing absurd in it; but that, on the contrary, there is a value of z to each value of a, which would fulfil the conditions of the question, were we but able to rectify the circle.

This assertion demonstrated.

(B) Let A B C, Pl. I, fig. 6, be the given arc, consisting of n quadrants, n being any positive number, whole or fractional; to this add the quadrant C P D, in which take the variable arc C E; and through E, from the centre O, draw O F, meeting the tangent C T, in F; put C O $= r$, O F $= s$, arc C E $= x$, corresponding tangent C F $= y$; then by the nature of the circle, as $s^2 : r^2 :: \dot{y} : \dot{x}$; but s^2 is greater than r^2, therefore $\dot{y}$ is greater than $\dot{x}$, consequently y increases faster than x: moreover, when $y = 0$, $a + x = a + 0 = a$, in which case $a + x$ is infinitely greater than y, but y increases faster than x, and exceeds it infinitely, when $x =$ the quadrant C P D; consequently, by prime and ultimate ratios, there is a point P betwixt C and D, which cuts off an arc C P, or z, the tangent of which, C T, or $t =$ the arc A C P, i. e. $a + z = t$.

The geometrical form of the problem.

(C) When A C P $=$ C T, the sector A O P $=$ triangle C O T; from each take the common sector C O P, and the sector A O C $=$ the space C P T; hence the problem, treated geometrically, assumes this form; to find a point T in the tangent C F, produced if necessary, from which if T O be drawn to the centre, it shall give the space C P T $=$ the given sector A O C, for this construction will evidently make the tangent C T $=$ the arc A C P.

When $a = 0$, $z = 0$, or is infinitely small.

(D) If the arc A B C $= 0$, the sector A O C $= 0$; therefore the space C P T $= 0$, by (C); hence the arc C E P $= 0$, i. e. when $a = 0$, z is evanescent; consequently, the problem is impossible, unless a be a finite magnitude.

n not restricted to odd numbers.

(E) It appears from (B) and (D), that z is a real, not an imaginary arc, provided a be a finite magnitude, which may be expressed by n Q, Q being a quadrant, and n a positive number, either whole or fractional. This conclusion however is rejected by a celebrated mathematician, who intimates, that n is always an odd number; the passage containing his opinion is here quoted.

"Invenire omnes arcus, qui tangentibus suis sint æquales.

"*Solutio.* Primus arcus, hac proprietate præditus, est infinite parvus. Tum in secundo quadrante, quia hic tangentes sunt negativæ, datur nullus istiusmodi arcus; in tertio vero quadrante dabitur unus 270° aliquanto minor; porro dabuntur ejusmodi arcus in quinto, septimo, &c."

The reason assigned for n being an odd number in this quotation is derived from the supposition, that all the tangents are negative in even quadrants. To examine this reason on its own principles, let us suppose the given arc z to begin at B in the figure, not at A, as in article (B); then $a =$ arc B C $=$ a quadrant, and $n=1$, this makes C E D the second or an even quadrant: through B and C draw the tangents B R, T C R, and the angle T R B is manifestly right; that is, C T is perpendicular to B R, but B R is a positive tangent, because B C is an odd quadrant, and T C has been shown to be perpendicular to B R, which invalidates the reason why n should be odd, because the relations of perpendicular lines are not the relations of $+$ and $-$, or positive and negative quantities.

Mutual relations of a & z.

(F) Certain mutual relations of a and z may be investigated in the following manner. Suppose these arcs to vary, so as to preserve the equation $a+z=t$, and we have $\dot a+\dot z=\dot t$; hence $\dot a=\dot t-\dot z$, but $\dot t=\frac{s^2\dot z}{r^2}=(t^2+r^2)\times\frac{\dot z}{r^2}$, therefore $\dot a=\frac{t^2\dot z}{r^2}$. Now when z is less than $\frac{Q}{2}$, t^2 is less than r^2, and $\dot a$ is less than $\dot z$; but a and z begin together by (D), or the present article; therefore, when r is a small arc, it is greater than a, and $z-a$ is the greatest, when the angle C O P $=45°$; on the contrary, when $z=Q$ it is finite, and a maximum, and a is also a maximum, but infinite; hence we have the following equations, $a=(n-1)\times z$, $t=a+z=nz$, where n is any number, whole or fractional, greater than unity.

z found by approximation, a being given.

(G) These things being premised, we may find z by approximation to any given value of a, thus: put the given arc $a+Q$, or A C D $=g$, $r=1$, arc P D $=v$, its tangent or cotangent of $z=q$; then by the problem $g-v=t$, but by trigonometry $t\times q=r^2=1$, and $q=v+\frac{v^3}{3}+\frac{2v^5}{15}+\frac{17v^7}{315}+\frac{62v^9}{2835}+\frac{1382v^{11}}{155925}+\frac{21844v^{13}}{6081075}+\frac{929569v^{15}}{638512875}+$ &c. Now put $\frac{1}{3}$, $\frac{2}{15}$, $\frac{17}{315}$, $\frac{62}{2835}$, &c. $=b, c, d, e,$ &c. respectively; and we have $tq=gv-v^2+gbv^3-bv^4+gcv^5-cv^6+gdv^7-dv^8+gev^9-ev^{10}$, &c. $=1$; and by reverting the series, as in Emerson's Algebra, page 171, Problem LXII) we get $v=\frac{1}{g}$

$$\frac{1}{g}+\frac{1}{g^3}+\frac{2-g^2 b}{g^5}+\frac{5-4g^2 b}{g^7}+\frac{14-15g^2 b+3g^4 b^2-g^4 c}{g^9}+\frac{42-56g^2 b+28g^3 b-6g^4 c-7g^4 b}{g^{11}},$$ &c.; but v is the complement of z; therefore $z=Q-v=1{\cdot}570796-v$ nearly.

Example to the last paragraph.

(H) For example, let $a=9\times Q$; then $g=10\times Q=15{\cdot}70796$; whence $\frac{1}{g}={\cdot}063662$; $\frac{1}{g^3}={\cdot}000266$; $\frac{2-g^2 b}{g^5}={\cdot}000089$. Stopping here we get $v=\frac{1}{g}+\frac{1}{g^3}+\frac{2-g^2 b}{g^5}={\cdot}063839$; but arc C P $=Q-v=1{\cdot}506957$: dividing this by ·017453 the length of a degree to the radius unity, we get 86° 20′ 37″ for the angle subtended by the arc C P, or z.

v in the last article is too great.

(K) Since $t=g-v=15{\cdot}644121$; and $q=v+\frac{v^3}{3}+\frac{2v^5}{15}$ &c. $={\cdot}063992$; we have $t\times q=1{\cdot}001098$, &c.; but $t\,q=1$ universally, which shows, that $g-v$ exceeds the true value of t, or $\frac{1}{q}=15{\cdot}62386$, &c.; therefore v is a little too great, which makes z too little: but it is to be remembered, that the true place of P has been nearly found in an even quadrant.

z found when $t=n\,z$.

(L) It appears from (F), that we may put $t=n\,z$; moreover, we have (by trigonometry) $q\,t=r^2=1$, hence $q\,z=\frac{1}{n}$; put $z=Q-v$, Q being a quadrant or 1·570796; also, put $q=v+b\,v^3+c\,v^5+d\,v^7$, &c., as in (G), and we have $Q\,v-v^2+Q\,b\,v^3-b\,v^4+Q\,c\,v^5-c\,v^6$ &c. $=\frac{1}{n}$; and by reversing the series, we get $v=\frac{1}{Q\,n}+\frac{1}{Q^3 n^2}+\frac{2-Q^2 b}{Q^5 n^3}+\frac{5-4Q^2 b}{Q^7 n^4}$, &c., where n is always greater than unity; but when v is known, z is given $=Q-v$.

Example to the last paragraph.

(M) Put $n=100$, then $\frac{1}{Q\,n}={\cdot}000636$, $\frac{1}{Q^3 n^2}={\cdot}000028$; stopping here, we get $v={\cdot}000664$, and $z=1{\cdot}570132$, or 89° 57′ 48″.

(N) When a is but small, the series given in (G) converges but slowly, in which case the following approximation may be used. Since $a + z = t$, $z = t - a$, but $z = t - \frac{t^3}{3} + \frac{t^5}{5} - \frac{t^7}{7} + \frac{t^9}{9}$ &c.; hence $\frac{t^3}{3} - \frac{t^5}{5} + \frac{t^7}{7} + \frac{t^9}{9}$ &c. $= a$; put $3 \times a = p$, and we have, by Emerson's Algebra, page 76, $t = p^{\frac{1}{3}} + \frac{p}{5} + \frac{3p^{\frac{5}{3}}}{175} - \frac{59p^{\frac{7}{3}}}{525}$, &c.

(P) For example, let $a = {\cdot}009$, then $p = {\cdot}027$, and $p^{\frac{1}{3}} = {\cdot}3$, and substituting the successive powers of ·3, or $p^{\frac{1}{3}}$, we get $t = {\cdot}3054157$, and $z = t - a = {\cdot}2964157$.

(Q) When n, in (L), consists of a unit and a small fraction, we may also approximate to the value of t, by help of the two values of z, viz. $\frac{t}{n}$, and $t - \frac{t^3}{3} + \frac{t^5}{5} - \frac{t^7}{7}$, &c.; from which we get $\frac{t^2}{3} - \frac{t^4}{5} + \frac{t^6}{7} - \frac{t^8}{9}$, &c. $= n - \frac{1}{n}$: call the small fraction, $\frac{n-1}{n}$, w; and we have by reversion, $t^2 = 3 \times w + 5{\cdot}4 \times w^2 + 7{\cdot}86857139 \times w^3 + 10{\cdot}33714521 \times w^4 + 12{\cdot}8037915 \times w^5$, &c.

II.

On the Inconvertibility of Bark into Alburnum. By THOMAS ANDREW KNIGHT, *Esq. F. R. S.* *In a Letter to Sir* JOSEPH BANKS, *K. B. P. R. S.*[*]

My dear SIR,

IN a letter which I had the honour to address to you in the end of last year[†], I endeavoured to prove, that the matter, which composes the bark of trees, previously exists in the cells both of their bark and alburnum in a fluid state; and that this fluid, even when extravasated, is capable

Matter that composes the bark of trees fluid in the cells of both bark & alburnum.

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[*] Phil. Trans. for 1808, P. I, p. 103.

[†] Phil. Trans. 1807; or Journal, vol. XIX, p. 241.

ble of changing into a pulpous and cellular, and ultimately a vascular substance; the direction taken by the vessels being apparently dependent on the course, which the descending fluid sap is made to take*. The object of the present Memoir is to prove, that the bark thus formed always remains in the state of bark, and that no part of it is ever transmuted into alburnum, as many very eminent naturalists have believed.

Always remains bark.

Experiments on the apple and crab.

Their bark mutually inosculated.

Having procured, by grafting, several trees of a variety of the apple und crab tree, the woods of which were distinguishable from each other by their colour, I took off, early in the spring, portions of bark of equal length, from branches of equal size, and I transposed these pieces of bark, enclosing a part of the stem of the apple tree with a covering of the bark of the crab tree, which extended quite round it, and applying the bark of the apple tree to the stem of the crab tree in the same manner. Bandages were then applied to keep the transposed bark and the alburnum in contact with each other; and the air was excluded by a plaster composed of bees wax and turpentine, and with a covering of tempered clay.

Interior surfaces different.

The interior surface of the bark of the crab tree presented numerous sinuosities, which corresponded with similar inequalities on the surface of the alburnum, occasioned by the former existence of many lateral branches. The interior surface of the bark of the apple tree, as well as the external surface of the alburnum, was, on the contrary, perfectly smooth and even. A vital union soon took place between the transposed pieces of bark, and the alburnum and bark of the trees to which they were applied; and in the autumn it appeared evident, that a layer of alburnum had

Union took place.

A layer of al-

Extravasated animal fluids become vascular.

* I had observed this circumstance in many successive seasons; but I was not by any means prepared to believe, that such an arrangement could take place in the coagulum afforded by an extravasated fluid; and I am indebted to Mr. Carlisle for having pointed out to me many circumstances in the motion and powers of the blood of animals, which induced me to give credit to the accuracy of my observations; and to that gentleman and to Mr. Home I have also subsequently to acknowledge many obligations.

burnum formed beneath.

had been, in every instance, formed beneath the transposed pieces of bark, which were then taken off.

Alburnum did not adapt its surface to that of the foreign bark.

Examining the organization of the alburnum, which had been generated beneath the transposed pieces of bark of the crab tree, and which had formed a perfect union with the alburnum of the apple tree, I could not discover any traces of the sinuosities I had noticed; nor was the uneven surface of the alburnum of the crab tree more changed by the smooth transposed bark of the apple tree. The newly generated alburnum, beneath the transposed bark, appeared perfectly similar to that of other parts of the stock, and the direction of the fibres and vessels did not in any degree correspond with those of the transposed bark *.

Surface of the alburnum scraped.

Repeating this experiment, I scraped off the external surface of the alburnum in several spaces, about three lines in diameter, and in these spaces no union took place between the transposed bark and the alburnum of the stock, nor was there any alburnum deposited in the abraded spaces; but the newly generated cortical and alburnous layers took a circular, and rather elliptical, course round those spaces, and appeared to have been generated by a descending fluid, which had divided into two currents when it came into contact with the spaces from which the surface had been scraped off, and to have united again immediately beneath them.

New cortical & alburnous layer formed.

In each of these experiments, a new cortical and alburnous layer was evidently generated; and apparently by the same means that similar substances were generated beneath a plaster composed of bees wax and turpentine, in former experiments †; and the only obvious difference in the result appears to be, that the transposed and newly generated bark formed a vital union with each other: and it is sufficiently evident,

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Duhamel's experiment defective.

* Duhamel having taken off, and immediately replaced, similar pieces of the bark of young elms, subsequently found, that the alburnum, which was generated beneath such pieces of bark, had not formed any union with the alburnum of the tree beneath it. But this great naturalist did not employ ligatures of sufficient power, to bring the bark and alburnum into close contact, or the result would have been different.

† Phil. Trans. for 1807; or Journal, vol. XIX, p. 243.

evident, that if bark of any kind was converted into alburnum, it must have been that newly generated. For it can scarcely be supposed, that the bark of a crab tree was transmuted into the alburnum of an apple tree, or that the sinuosities of the bark of the crab tree could have been obliterated, had such transmutation taken place. There is not, however, any thing in the preceding cases calculated to prove, that the newly generated bark was not converted into alburnum; and the elaborate experiments of Duhamel sufficiently evince the difficulty of producing any decisive evidence in this case; nevertheless I trust, that I shall be able to adduce such facts as, in the aggregate, will be found nearly conclusive.

Young shoots of oak. No transmutation of bark into alburnum.

Examining almost every day, during the spring and summer, the progressive formation of alburnum in the young shoots of an oak coppice, which had been felled two years preceding, I was wholly unable to discover any thing like the transmutation of bark into alburnum. The commencement of the alburnous layers in the oak *(quercus robur)* is distinguished by a circular row of very large tubes. These tubes are of course generated in the spring; and during their formation, I found the substance through which they passed to be soft and apparently gelatinous, and much less tenacious and consistent than the substance of the bark itself; and, therefore, if the matter which gave existence to the alburnum previously composed the bark, it must have been, during its change of character, nearly in a state of solution. But it is the transmutation of one organized substance into the other, and not the identity only of the matter of both, for which the disciples of Malpighi contend; and if the fibres and vessels of the bark really became those of the alburnum, a very great degree of similarity ought to be found in the organization of those substances. No such similarity, however, exists; and not any thing at all corresponding with the circular row of large tubes in the alburnum of the oak is discoverable in the bark of that tree. These tubes are also generated within the interior surface of the bark, which is well defined; and during their formation the vessels of the bark are distinctly visible, as different organs; and had the one been transmuted into the other, their progressive

Barks of wych elm and ash differ essentially, but not the alburnum, or wood.

progressive changes could not, I think, possibly have escaped my observation. Nor does the organization of the bark in other instances in any degree indicate the character of the wood, that is generated beneath it: the bark of the wych elm *(ulmus montana)* is extremely rough and fibrous; and it is often taken from branches of six or eight years old, to be used instead of cords; that of the ash *(fraxinus excelsior)* on the contrary, when taken from branches of the same age, breaks almost as readily in any one direction as in another, and scarcely presents a fibrous texture; yet the alburnum of these trees is not very dissimilar, and the one is often substituted for the other in the construction of agricultural instruments.

Mirbel's theory.

Mirbel has endeavoured to account for the dissimilar organization of the bark, and of the wood into which he conceives it to be converted, by supposing, that the cellular substance of the bark is always springing from the alburnum, while the tree is growing; and that it carries with it part of the tubular substance *(tissu tubulaire)* of the liber, or interior bark. These parts of the interior bark, which are thus removed from contact with the alburnum, he conceives to constitute the external bark or cortex, while the interior part of the liber progressively changes into alburnum.

Objections to this theory.

But if this theory (which I believe I have accurately stated, though I am not quite certain, that I fully comprehend its author*) were well founded, the texture of the alburnum must surely be much more intricate and interwoven than it is, and its tubes would lie less accurately parallel with each other than they do: and were the fibrous substance of the bark progressively changing into alburnum, the bark must of necessity be firmly attached to the alburnum during the spring and summer by the continuity, and indeed identity of the vessels and fibres of both these substances. This, however, is not in any degree the case, and the bark is in those seasons very easily separated from the alburnum; to which it appears to be attached by a substance that is apparently rather gelatinous than fibrous or vascular:

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* Chap. III, Article 5, *Traité d'Anatomie et de Physiologie Végétale.*

Objection to Malpighi.

and the obvious fact, that the adhesion of the cortical vessels and fibres to each other is much more strong than the adhesion of the bark to the alburnum, affords another circumstance almost as inconsistent with the theory of Malpighi, as with that of Mirbel.

Duhamel's experiment of wire in the alburnum.

Many of the experiments of Duhamel are, however, apparently favourable to the theory of Malpighi, respecting the conversion of bark into alburnum; and Mirbel has cited two, which he appears to think conclusive*. In the first of these Duhamel shows, that pieces of silver wire, inserted in the bark of trees, were subsequently found in their alburnum. But Duhamel himself has shown, with his usual acuteness and candour, that the evidence afforded by this experiment is extremely defective; and he declares himself to be uncertain, that the pieces of wire did not, at their first insertion, pass between the bark and the alburnum; in which case they would necessarily have been covered by every successive layer of alburnum, without any transmutation of bark into that substance†.

His experiment of a peach bud and bark inserted in a plum stock.

In the second experiment cited by Mirbel, Duhamel has shown, that when a bud of the peach tree, with a piece of bark attached to it, is inserted in a plum stock, a layer of wood perfectly similar to that of the peach tree will be found, in the succeeding winter, beneath the inserted bark. The statement of Duhamel is perfectly correct: but the experiment does not by any means prove the conversion of bark into wood; for if it be difficult to conceive (as he remarks) that an inserted piece of bark can deposit a layer of alburnum, it is at least as difficult to conceive, how the same piece of bark can be converted into a layer of alburnum of more than twice its own thickness (and the thickness of the alburnum deposited frequently exceeds that of the bark in this proportion), without any perceptible diminution of its own proper substance. The probable operation of the inserted bud, which is a well organized plant, at the period when it becomes capable of being transposed with success, appears also, in this case, to have been overlooked; for I found, that when I destroyed the buds in the succeeding winter, and left the bark which belonged to them uninjured, this bark no longer possessed any power to generate alburnum. It nevertheless continued to live, though perfectly inactive, till it became covered by the successive alburnous layers of the stock; and it was found many years afterwards enclosed in the wood. It was, however, still bark, though dry and lifeless, and did not appear to have made any progress towards conversion into wood.

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* Chap. III, Article 5.

† *Physique des Arbres*, Lib IV, Ch. III.

No facts to prove, that bark is converted into alburnum.

In the course of very numerous experiments, which were made to ascertain the manner in which vessels are formed in the reproduced bark *, many circumstances came under my observation, which I could adduce in support of my opinion, that bark is never transmuted into alburnum; but I do not think it necessary to trouble you with an account of them; for though much deference is certainly due to the opinions of those naturalists, who have adopted the opposite theory, and to the doubts of Duhamel, I am not acquainted with a single experiment, which warrants the conclusions they have drawn; and I think, that, were bark really transmuted into alburnum, its progressive changes could only have escaped the eyes of prejudiced or inattentive observers. In the course of the ensuing spring, I hope to address to you some observations respecting the manner in which the alburnum is generated.

I am, my dear Sir,

Your most obliged obedient servant,

THOMAS AND. KNIGHT.

Elton, *Dec.* 29, 1807.

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* Phil. Trans. for 1807; or Journal, vol. XIX, p. 241.

III.

III.

Account of a Mine of Zinc Ore, and its Application as a Paint. By the Right Hon. Lord RIBBLESDALE, *of Gisburne Park, Yorkshire*[*].

SIR,

Ore of zinc as a white paint. HEREWITH I send you a specimen of white paint, which, for the sake of humanity, I trust will be found a complete substitute for that baneful article white lead.

I have used this paint for twelve years upon my house, paling, doors, &c. It is of a delicate stone tint, but becomes equal in colour by time to the best white lead, and for durability, for never blistering, and for body and adhesion infinitely superior to it.

If the specimen (which is the average of what may be ordinarily obtained, although for particular purposes it may be produced much finer), should meet with the approbation of the Society of Arts, &c., I shall at any time, with the greatest pleasure, at their request, render them all the information upon the subject in my power. I have painted four or five years ago a vessel, which is now in his Majesty's service, with this paint, and nothing can exceed the resistance which this paint makes to all the effects of salt water to decompose it.

Resists salt water.

I have the honour to be, with much respect,

Sir,

Your obedient humble servant,

RIBBLESDALE.

Additional Communication by Lord Ribblesdale, on his Ore of Zinc.

Mines. The mines are situate at Mallam Moors, in Craven, Yorkshire, and in an extent of country of eleven or twelve thousand acres of land belonging to his Lordship; where the mineral is found, there were formerly copper mines.

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[*] Trans. of the Soc. of Arts for 1807, p. 35. Although this ore of zinc did not appear, upon trial by various persons, fully to answer the purposes of white lead, as a basis for paint, yet it possessed sufficient merit, to induce the Society to vote their silver medal to his Lordship.

This article is found in caverns, about eight fathoms from the surface of the earth. The mineral lies in strata, along the bottoms of these caverns, which strata are from three to six feet thick, and the best coloured mineral, or whitest, lies the lowest. On the upper part of the caverns are beautiful stalactites of great hardness.

One of the caverns wherein it is found is one hundred and four yards, another forty-four, and a third eighty-four yards in length, and about fourteen yards wide.

Surrounding stones vitrified.

His Lordship supposes this mineral has been sublimed by a volcano, as the stones surrounding it have been vitrified.

Tried as a paint 12 years ago.

Before used for making brass.

The mineral was first tried as a paint twelve years ago; it was previously sold, and continues to be sold to make brass at Birmingham and other places. He has sold upwards of two thousand tons, at from five to ten pounds per ton, for making brass, when mixed with copper.

Its excellencies.

His Lordship stated, that it has answered well for house painting externally, and the whiteness improves by time; that it will in painting cover a much larger surface than white lead paint, and he supposes it will do half as much more work; that it forms a body on the wood so hard as to resist the edge of an adze; and that it forms a strong cement betwixt two boards painted with it.

That it will never peel off; that the oil paint on palings withstands the effect of moisture; and that it will mix as a basis with all other colours.

Cheap.

His Lordship added, that the price will not exceed that of white lead; on the contrary, he thinks, that, except in the finest preparations, upon an average it will come considerably lower.

Dear Sir,

The ore very pure.

PERMIT me again to assure the Society, that the body of my paint is equal to white lead, and that the ore itself is so pure, and is found in the mine so little mingled with any other substance, that I do not lose two pounds of the colour in a ton of the ore.

I remain, dear Sir,
Your very humble servant,
RIBBLESDALE.

IV.

On Oxalic Acid. By THOMAS THOMSON, *M. D. F. R. S. Ed. Communicated by* CHARLES HATCHETT, *Esq., F. R. S.*[*]

Oxalic acid discovered by Scheele.

OXALIC acid, from the united testimony of Ehrhart, Hermbstadt, and Westrumb, appears to have been discovered by Scheele; but it is to Bergman that we are indebted for the first account of its properties. He published his dissertation on it in 1776, and since that time very little has been added to the facts contained in his valuable treatise. Chemists have chiefly directed their attention to the formation of that acid, and much curious and important information has resulted from the experiments of Hermbstadt, Westrumb, Berthollet, Fourcroy, Vauquelin, &c. but the properties of the acid itself have been rather neglected. My object in the following pages is not to give a complete history of the properties of oxalic acid, but merely to state the result of a set of experiments, undertaken with the view of ascertaining different particulars respecting it, which I conceived to be of importance.

Little attention paid to its properties.

I. *Water of Crystallization.*

Its water of crystallization.

Oxalic acid is usually obtained in transparent prismatic crystals more or less regular; these crystals contain a portion of water, for when moderately heated they effloresce and lose a part of their weight, which they afterwards recover when left exposed in a moist place. When cautiously heated on a sand bath they fall to powder, and lose about a third of their weight. But as the acid is itself volatile, it is not probable that the whole of this loss is water. To ascertain the quantity of water contained in these crystals I had recourse to the following method.

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[*] Philos. Trans. for 1807, p. 63.

The acid precipitated by muriate of lime;

1. Seventy grains of crystallized oxalic acid were dissolved in 600 grains of water, constituting a solution which weighed 670 grains.

Fifty grains of pure carbonate of lime, in the state of calcareous spar, were dissolved in muriatic acid; this solution was evaporated to dryness, to get rid of the excess of acid, and the residue redissolved in water.

As the muriatic acid set loose holds the last portions in solution,

Into this muriate of lime the solution of oxalic acid was dropped by little and little as long as any precipitate fell, and the oxalate of lime thus formed was separated by the filter. Pure oxalic acid is not capable of precipitating the whole lime from solution of muriate of lime, the muriatic acid evolved being always sufficient to retain the last portions in solution.

this saturated by ammonia.

It was necessary to get rid of this excess of acid; the method which appeared the least exceptionable was to saturate the muriatic acid with ammonia; accordingly when the oxalic acid ceased to occasion any farther precipitate, I cautiously added pure ammonia, till the liquid ceased to produce any effect upon vegetable blues. A copious additional precipitate of oxalate of lime was thus obtained. Oxalic acid was now added again as long as it rendered the liquid muddy. By thus alternately having recourse to the acid solution, and to ammonia, and by adding both with great caution to avoid any excess, I succeeded in separating the whole of the lime, without using any sensible excess of oxalic acid.

558 grains of the acid solution were employed, a quantity which is equivalent to 58·3 grains of the crystallized acid.

2. The oxalate of lime, after being well washed and drained, and exposed for a week to the open air, at a temperature of about 60°, weighed 76 grains; but upon being left on the sand bath for some hours in a temperature between 200° and 300°, its weight was reduced to 72 grains.

3. These 72 grains of dry oxalate of lime were put into an open platinum crucible, and gradually heated to redness.

By these means they were reduced to 49·5 grains, which proved to be carbonate of lime. The crucible was now exposed to a violent heat in a forge. Nothing remained but a quantity of pure lime weighing 27 grains.

72 dry oxalate contain 27 lime.

4. From this experiment we learn, that 72 grains of dry oxalate of lime contain 27 grains of lime. Of consequence, the oxalic acid in this compound must be 45 grains. But the weight of crystallized oxalic acid actually used was 58·3 grains, a quantity which exceeds the whole acid in the oxalate by 13·3 grains. These 13·3 grains are the amount of the water of crystallization, which either did not unite with the salt, or was driven off by the subsequent exposure to heat. Hence crystallized oxalic acid is composed of

Crystals of oxalic acid contain ·23 water.

Real acid	45·	equivalent to	77
Water........	13·3		23
	58·3		100

So that the crystals of oxalic acid contain very nearly the fourth part of their weight of water*.

II. *Alkaline and Earthy Oxalates.*

Oxalate of lime, 62·5 acid, 37·5 base.

1. The preceding experiment gives us likewise the composition of oxalate of lime. This salt, when merely dried in the open air, still retains a portion of water, which may

be

This proportion confirmed by an experiment of Vauquelin.

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* Vauquelin in a late dissertation on cinchona, marked with that profound skill which characterizes all the productions of this illustrious chemist, has mentioned incidentally, that the crystals of oxalic acid contain about half their weight of water. He dissolved 100 parts of cinchonate of lime in water, and precipitated by means of oxalic acid; 22 parts of crystallized oxalic acid were necessary; and the oxalate of lime formed weighed 27 grains. From this experiment he draws the conclusion which I have stated (See Ann. de Chimie, lix, 164; or our Journal, vol. XIX, p. 213). But this ingenious chemist does not seem to have been aware of the real composition of oxalate of lime. 27 grains of that salt are composed very nearly of 10 grains of lime and 17 grains of acid. But the weight of the crystals used by Vauquelin was 22; the difference, 5, is obviously the water of crystallization in 22 grains of the crystals. But if 22 grains contain 5 of water, it is obvious, that 100 contain very nearly 23. So that his experiment in reality coincides with mine.

be driven off by artificial heat. It is necessary to know, that it parts with this water with considerable difficulty, so that a long exposure on the sand or steam bath is necessary, to get it thoroughly dry. It afterwards imbibes a little water, if it be left in a moist place. Well dried oxalate we have seen is a compound of

Dried with difficulty.

Acid	45	or per cent, 62·5	acid.
Base	27	37·5	base.
	72	100	

Though the oxalate of lime dried spontaneously can scarcely be considered as always in the same state, yet as the difference in the portion of water which it retains is not great, provided it be dried slowly in the temperature of 60°, and in a dry place, it may be worth while to state its composition. It is as follows:

Dried slowly at 60° contains 0·53 water.

Acid	45	or per cent 59·2	acid.
Base	27	35·5	base.
Water	4	5·3	water.
	76	100·0	

When rapidly dried, as by pressing it between the folds of filtering paper, it is apt to concrete into hard lumps, which retain more moisture. In this state I have sometimes seen it retain 10 per cent of water after it appeared dry.

Dried rapidly sometimes ·1 of water.

Bergman states the composition of oxalate of lime as follows:

Bergman's statement.

Acid	48
Lime	46
Water	6
	100*

His method was to dissolve a determinate quantity of calcareous spar in nitric acid, and then to precipitate the lime by oxalic acid. 100 parts of calcareous spar thus dissolved, require, according to him, 82 parts of crystallized acid to precipitate them. But there must have been some mistake

His method.

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* Opusc. I, 262.

in this experiment; for, according to my trials (provided the nitric acid be carefully naturalized by ammonia as it is evolved), no less than 117 grains of oxalic acid would have been required, and at least 145 grains of oxalate of lime would have been obtained instead of the 119, which was the result of Bergman's experiment. It is obvious, that Bergman did not precipitate all the lime. He added oxalic acid till it ceased to produce any effect on the solution from the great excess of nitric acid evolved; and then took it for granted, that all the lime was separated. But had he added ammonia, he would have got an additional quantity of oxalate of lime, and the precipitation would have recommenced upon adding more oxalic acid. This explanation accounts in a satisfactory manner for the difference between Bergman's statement of the composition of oxalate of lime, and mine.

Cause of his mistake.

The preceding analysis verified.

2. Though the preceding experiment was made with care, yet as some of the most important of the following observations in some measure rest upon the analysis of oxalate of lime, I thought it worth while to verify that analysis in the following manner.

100 grains of crystallized oxalic acid were dissolved in 1000 grains of water, making a solution which weighed 1100 grains.

It is obvious, that every 100 grains of the above solution contained 9·09 grains of crystals of oxalic acid, equivalent, according to the preceeding analysis, to 7 grains of real acid.

100 grains of this solution were gradually mixed with lime water, till the liquid ceased to produce any change on vegetable blues. The oxalate of lime thus formed, being well dried, weighed 11·2 grains. Exposed to a violent heat in a platinum crucible, this salt left 4·2 grains of pure lime. Hence it was composed of

7	acid, or per cent	62·5	acid
4·2	lime	37·5	base
11·2		100·0	

Thus we have obtained exactly the same result as in the former experiment, both as far as relates to the composition of oxalate of lime, and likewise to the proportion of water of crystallization in crystallized oxalic acid.

Water dissolves $\frac{1}{758}$ lime.

The lime water necessary to saturate the acid amounted to 3186 grains. Hence, it contained only $\frac{1}{758}$th of its weight of lime.

Oxalates of barytes and strontian.

3. The oxalates of barytes and strontian are white, tasteless powders, which may be obtained by mixing oxalate of ammonia with the muriates of these alkaline earths. It is said, that these earths are capable of forming soluble superoxalates with this acid; but I have not tried the experiment. These oxalates, as well as oxalate of lime, are partially soluble in the strong acids.

Oxalate of magnesia.

4. Oxalate of magnesia is a soft white powder, bearing a considerable resemblance to oxalate of lime. It is tasteless, and not sensibly soluble in water; yet when oxalate of ammonia is mixed with sulphate of magnesia, no precipitate falls; but if the solution be heated and concentrated sufficiently, or if it be evaporated to dryness, and redissolved in water, in both cases the oxalate of magnesia separates in the state of an insoluble powder.

Oxalate of potash.

5. Oxalate of potash readily crystallizes in flat rhomboids, commonly terminated by dihedral summits. The lateral edges of the prism are usually bevilled. The taste of this salt is cooling and bitter. At the temperature of 60° it dissolves in thrice its weight of water. When dried on the sand bath, and afterward exposed in a damp place, it absorbs a little moisture from the atmosphere.

Salt of sorrel.

This salt combines with an excess of acid, and forms a superoxalate, long known by the name of *salt of sorrel*. It is very sparingly soluble in water, though more so than tartar. It occurs in commerce in beautiful 4-sided prisms attached to each other. The acid contained in this salt is very nearly double of what is contained in oxalate of potash. Suppose 100 parts of potash; if the weight of acid necessary to convert this quantity into oxalate be x, then $2x$ will convert it into superoxalate.

Oxalate of soda.

6. Oxalate of soda readily crystallizes. Its taste is nearly the same as that of oxalate of potash. When heated, it falls to powder, and loses the whole of its water of crystallization. Soda is said to be capable of combining with an

excess of acid, and of forming a superoxalate. I have not tried the experiment.

Oxalate of ammonia.

7. Oxalate of ammonia is the most important of all the oxalates, being very much employed by chemists to detect the presence of lime, and to separate it from solutions. It crystallizes in long trasparent prisms, rhomboidal, and terminated by dihedral summits. The lateral edges are often truncated, so as to make the prism 6 or 8-sided. Sometimes the original faces of the prism are nearly effaced.

The taste of this salt is bitter and unpleasant, somewhat like that of sal ammoniac. At the temperature of 60°, 1000 grains of water dissolve only 45 grains of this salt. Hence, 1000 grains of saturated solution of oxalate of ammonia contain only 43·2 grains of this salt. The specific gravity of this solution is 1·0186. As it may be useful to know the weight of this salt contained in solutions of different specific gravities, I have thought it worth while to construct the following table:

Specific gravity of solution of oxalate of ammonia.

Weight of oxalate of ammonia in 100 parts of the solution	Specific gravity of the solution at 60°	Weight of oxalate of ammonia in 100 parts of the solution	Specific gravity of the solution at 60°.
4·32	1·0186	1·5	1·0075
4·	1·0179	1·	1·0054
3·5	1·0160	0·5	1·0030
3·	1·0142	0·4	1·0024
2·5	1·0120	0·3	1·0018
2·	0·0095	0·2	1·0012
		0·1	1·0006

Method of determining the combustion of the oxalates.

8. To determine the composition of these salts, I took seven different portions of a diluted oxalic acid solution, each weighing 100 grains, and containing 7 grains of real oxalic acid. To each of these portions I added respectively potash, soda, ammonia, barytes water, strontian water, and lime water, till it ceased to produce any change. The liquid was then evaporated to dryness, and the residue, after being well dried on the steam bath, was weighed. Each of these salts contained 7 grains of acid; the additional

weight I ascribed to the base. Hence I had the following table, which exhibits the weight of each salt obtained, and its composition deduced from that weight.

Salts.	Weight obtained	Composition	
		Acid	Base.
Oxalate of Ammonia	9·4	7	2·4
——— Magnesia*..	9·5	7	2·5
——— Soda	11·0	7	4·0
——— Lime	11·2	7	4·2
——— Potash	15·6	7	8·6
——— Strontian ..	17·6	7	10·6
——— Barytes	17·0	7	10·0

The composition of these salts reduced to 106 parts is given in the following table.

Component parts of the oxalates.

	Ox. of Ammonia	Ox. of Magnesia.	Ox. of Soda.	Ox. of Lime.	Ox. of Potash	Ox. of Strontian.	Ox of Barytes
Acid	74·45	73·68	63·63	62·50	44·87	39·77	41·16
Base	25·53	26·32	36·37	37·50	55·13	60·23	58·84
Total	100	100	100	100	100	100	100

But for practical purposes, it is more convenient to consider the acid as a constant quantity. The following table is constructed upon this plan.

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* The oxalate of magnesia was obtained by neutralizing the oxalic acid solution with ammonia, then mixing it with sulphate of magnesia, evaporating the solution to dryness, and washing the insoluble oxalate of magnesia with a sufficient quantity of water.

Component parts the acid being 100.

	Acid	Base.	Weight of Salt.
Oxalate of Ammonia··	100	34·12	134·12
——— Magnesia ··	100	35·71	135·71
——— Soda ······	100	57·14	157·14
——— Lime ······	100	60·00	160·00
——— Potash ····	100	122·86	222·86
——— Strontian ··	100	151·51	251·51
——— Barytes ····	100	142·86	242·86

Oxalates retain little if any water in a moderate heat,

9. In the preceding statement, no account has been taken of the water of crystallization, which might still remain attached to the salts, notwithstanding the heat to which they were exposed. There is reason to believe, however, that in most of them this water must be so small, that it may be overlooked without any great errour. Oxalates of soda and of ammonia, I have reason to believe, lose all their water of crystallization at a moderate heat. This is the case also with oxalates of lime and barytes; and I presume, that the oxalates of strontian and magnesia are not exceptions; but oxalate of potash retains its water much more obstinately. I believe that in this salt the weight of acid and of base are nearly equal, and that when dried in the temperature of 212°, it still retains nearly 10 per cent of water; but I have not been able to establish this opinion by direct experiment.

except that of potash.

Oxalate of strontian.

The composition of oxalate of strontian in the preceding table was so different from what I expected, that I repeated the experiment; but the result was the same. This induced me to combine strontian and oxalic acid in the following manner: 100 grains of a solution containing 7 grains of real oxalic acid were neutralized by ammonia, and the oxalic acid precipitated by means of muriate of strontian. The salt obtained weighed 12·3 grains; of course it was composed of

Acid	7	or	56·9	or	100
Base	5·3		43·1		75·7
	12·3		100·0		175·7

Thus

Two species. One with double the base of the other.

Thus it appears, that there are two oxalates of strontian, the first obtained by saturating oxalic acid with strontian water, the second by mixing together oxalate of ammonia and muriate of strontian. It is remarkable, that the first contains just double the proportion of base contained in the second.

III. *Decomposition of the Oxalates.*

Crystallized acid in part sublimes: in oxalates decomposed by heat.

1. When oxalic acid, in the state of crystals, is exposed to heat, it is only partially acted upon, a considerable portion escaping without alteration; but when an alkaline or earthy oxalate is heated, the acid remains fixed, till it undergoes complete decomposition. The new substances into which the acid is converted, as far as my experience goes, are always the same, what oxalate soever we employ. They are five in number; namely, *water*, *carbonic acid*, *carbonic oxide*, *carburetted hidrogen*, and *charcoal*.

Products.

Water.

2. The water is never quite pure. Though no sensible portion of oil can be perceived in it, yet it has always the peculiar smell of the water obtained during the distillation of wood; a smell which is usually ascribed to oil. It commonly shows traces of the presence of ammonia, changing vegetable blues to green, and smoking when brought near muriatic acid; but this minute portion of ammonia is probably only accidentally present. All the oxalates, which I decomposed by distillation, were obtained by double decomposition from oxalate of ammonia; and though they were washed with sufficient care, yet I think it not unlikely, that a minute portion of oxalate of ammonia might continue to adhere. Practical chemists know the extreme difficulty of removing every trace of a salt, with which another has been mixed.

Carbonic acid.

The carbonic acid remains partly combined with the base, which always becomes a carbonate, and partly makes its escape in the form of gas.

Carbonic oxide, carburetted hidrogen, and charcoal.

The carbonic oxide and carburetted hidrogen make their escape in the form of gas: the charcoal remains in the retort mixed with the base, to which it communicates a gray colour: the quantity of it depends in some measure upon the heat. If the oxalate was exposed to a very violent heat, no charcoal at all remains. Hence it probably acts upon the carbonic acid united to the base, converting it into carbonic oxide, as happens when a mixture of a carbonate and charcoal are heated.

Decomposition of oxalate of lime attentively examined.

3. I was induced to examine this decomposition with considerable attention, because I conceived, that it would furnish the means of estimating the composition of oxalic acid; and I pitched upon oxalate of lime, as the salt best adapted for the purpose I had in view. A determinate quantity of this salt was put into a small retort, and gradually heated to redness. This retort was connected with a pneumatic trough by means of a long glass tube, having a valve at its extremity, which allowed gas to issue out, but prevented any water from entering the tube. The experiment was repeated three times.

100 grs. yield 60 inches of gas.

4. A hundred grains of oxalate of lime, when thus heated, yield above sixty cubic inches of a gas, which is always a mixture of carbonic acid and inflammable air, nearly in the proportion of one part of the former to three and a half of the latter, reckoning by bulk. The specific gravity of the inflammable gas was 0·908, common air being 1·000; it burns with a blue flame, and, when mixed with oxigen, may be kindled by the electric spark. The loudness of the report depends upon the proportion of oxigen.

Mixed with oxigen & kindled by the electric spark

The smallest quantity of oxigen, with which it can be mixed, so as to burn by the electric spark, is $\frac{1}{5}$th; the combustion is very feeble, and is attended with no perceptible report. If the residue be washed in lime water, and mixed with $\frac{1}{5}$th of its bulk of oxigen, it may be kindled a second time: this may be repeated five times, after which the residue cannot be made to burn.

The combustion becomes more violent, and the report louder, as we increase the proportion of oxigen, and both are greatest when the oxigen is double the bulk of the gas. As we increase the dose of oxigen, the combustion becomes more and more feeble; and five parts of oxigen and one of gas form the limit of combustion on this side: for a mixture of six parts of oxigen and one of the inflammable air will not burn.

In

Results differ according to the proportions of oxigen.

In these experiments the results differ materially from each other, when the proportion of oxigen used is small and when it is great. I am not able at present to account for this difference, which holds not only with respect to this gas, but every compound inflammable gas, which I have examined. This difference makes it impossible to use both extremes of the series: I make choice of that in which the proportion of oxigen is considerable, as upon the whole more satisfactory. The best proportion is one part of the gas and two parts of oxigen. The oxigen ought not to be pure, but diluted with at least the third of its bulk of azote, unless the gas be much contaminated with common air.

I have elsewhere detailed the method, which I follow in analyzing gasses of this nature*. The following table exhibits the mean of a considerable number of trials of this gas with oxigen.

Mean result of the combustion.

Measures of inflammable air consumed.	Measures of oxigen consumed.	Carbonic acid formed.	Diminution of bulk.
100	91	93	98

That is to say, 100 cubic inches of the gas, when burnt, combine with 91 cubic inches of oxigen; there are produced 93 inches of carbonic acid; and after the combustion these 93 inches alone remain, the rest being condensed. Hence we conclude, that the other substance produced was water.

This result corresponds almost exactly with what would have been obtained, if we had made the same experiment upon a mixture of 70 measures of carbonic oxide, and 30 measures of carburetted hidrogen, as will appear from the following table.

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* See Journal, vol. XVI, p. 247.

Carbonic

late of lime heated in a retort.

cularly. Eighty-nine grains of well dried oxalate of lime were exposed in a small retort to a heat gradually raised to redness; the products were the following:

	Grains.
45·6 cubic inches of gas* weighing....	14·8
Water	6·4
Residue in retort	62·4
	83·6
Loss	5.4
Total	89·0

The loss is obviously owing to the gas, which filled the retort and tube when the experiment was concluded. We are warranted therefore to add it to the weight of the gaseous products obtained.

Now the gas was composed of

Carbonic acid ·· 10·5 cubic inches = 4·9 grains.
Inflammable air 35·1 .... = 9·9

so that one third of the weight was carbonic acid, and two thirds inflammable air. If we divide the 5·4 grains of loss in that proportion, we obtain 1·8 grain carbonic acid, and 3·6 grains of inflammable air. Adding these quantities to the weight obtained, we get for the weight of the whole gaseous product

Gaseous product.

	Grains.
Carbonic acid	6·7
Inflammable air ··	13·5
	20·2

The 62·4 grains of residue in the retort were composed of

Residuum.

	Grains.
Lime............	33·4
Carbonic acid	26·4
Charcoal	2·6
	62·4

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[*] The gas obtained measured 60 cubic inches, but 14·4 inches of these were found to be common air, which had previously filled the retort and tube; this quantity was therefore deducted.

Now it is clear, that the 89 grains of oxalate of lime were composed of

Lime·············	33·4
Acid·············	55·6
	89·0

The acid was completely decomposed and resolved into the following products:

Products of 55·6 grs. of acid.

Carbonic acid ····	33·1
Inflammable air ··	13·5
Water ···········	6·4
Charcoal ········	2·6
	55·6

Had the experiment been made upon 100 grains of oxalic acid instead of 55·6, it is clear, that the proportions would have been as follows.

Proportions of 100 parts.

Carbonic acid····	59·53
Inflammable air··	24·28
Water···········	11·51
Charcoal········	4·68
	100·00

The most remarkable circumstance attending the decomposition of oxalic acid by heat is the great proportion of carbonic acid formed; the quantity amounts to 6 tenths of the whole weight of acid decomposed.

Constituent principles of the acid.

As the composition of all these products of oxalic acid is known with considerable accuracy, it is obvious, that they furnish us with the means of ascertaining the constituents of that acid itself.

59·53 grains of carbonic acid are composed of

	Grains.
Oxigen ······	42·86
Carbon ······	16·67
	59·53

24·28 grains of the inflammable air, according to the analysis given in a preceding part of this paper, are composed of

	Grains.
Oxigen	11·96
Carbon	10·43
Hidrogen	1·89
	24·28

11·51 grains of water are composed of

Oxigen	9·87
Hidrogen	1·64
	11·51

As for the charcoal, though it probably contains both oxigen and hidrogen as well as carbon, yet as the proportion of the first two ingredients is probably very small, and as we have no means of estimating them, we must at present rest satisfied with considering it as composed of pure carbon.

When these different elements are collected under their proper heads, we obtain

	Oxigen.	Carbon.	Hidrogen.
In carbonic acid	42·86	16·67	
Inflammable air	11·96	10·43	1·89
Water	9·87		1·64
Charcoal		4·68	
	64·69	31·78	3·53

Elements.

Hence oxalic acid is composed of oxigen	64·69
carbon	31·78
hidrogen	3·53
	100·00

Confirmed by other experiments.

7. The result of two other experiments on oxalate of lime was very nearly the same as the preceding. The following

may be stated in round numbers as the mean of the whole. Oxalic acid is a compound of

Mean in round numbers.

Oxigen	64
Carbon	32
Hidrogen	4
	100

Elements according to Fourcroy and Vauquelin.

8. The only other analysis of oxalic acid, with which I am acquainted, has been given by Mr. Fourcroy, as the result of his own experiments, in conjunction with those of Vauquelin[*]. It is as follows:

Oxigen	77
Carbon	13
Hidrogen	10
	100

Their calculations erroneous.

It gave me considerable uneasiness to observe, that my experiments led to conclusions irreconcilable with those of chemists of such eminence and consummate skill; and it was not without considerable hesitation, that I ventured to place any reliance upon them. I am persuaded, however, that some mistake has inadvertently insinuated itself into their calculations; since the carbonic acid alone, formed during the distillation of oxalate of lime, contains considerably more carbon than the whole quantity, which they assign to the oxalic acid decomposed. Mr. Fourcroy informs us, that oxalic acid is converted into carbonic acid and water, when acted upon by hot nitric acid; and this decomposition seems to have been the method employed, to ascertain the proportion of the constituents of oxalic acid; but the numbers assigned by him do not correspond with this statement. For 10 parts of hidrogen require 60 of oxigen to convert them into water, and 13 of carbon require at least 33 of oxigen. So that instead of 77 parts of oxigen, there would have been required no less than 98, to convert the hidrogen and carbon into water and carbonic acid. It is true, that the surplus of oxigen may be conceived to be furnished by the nitric acid; but if this be admitted (and I have no doubt from experience, that the nitric acid actually does communicate oxigen), it is difficult to see how the constituents of oxalic acid could be determined by any such decomposition, unless the quantity of oxigen furnished by the nitric acid were accurately ascertained.

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[*] *Système de Connois. Chem.* VII, 224. Trans. VII, 306.

(To be concluded in our next.)

V.

Analysis of some Iron Ores in Burgundy and Franche-Comté, to which is added, an Examination of the Pig Iron, Bar Iron, and Scoriæ, produced from them. By Mr. VAUQUELIN *.

Ores, iron, scoriæ, and fluxes, collected for examination.

MR. Vauquelin, in the year 1805, having visited various iron works in Burgundy, collected several specimens of ores, pig iron, bar iron, scoriæ, and fluxes, for the purpose of subjecting them to chemical analysis, in order to ascertain, whether it were possible to know from a comparison of their composition, what takes place in the processes, to which iron ores and cast iron are subjected. We shall give here the leading results of this able chemist's labours, and the particulars of some of the processes he employed to obtain these results.

I. *Chemical examination of some fluor spars.*

Fluor spar employed as a flux at Drambon.

The spar employed as a flux at the mine of Drambon, in the department of Côte-d'Or, is of a yellowish white, and tolerably hard. It dissolves with effervescence in nitric acid, and leaves a yellowish residuum, amounting to about a fifth of its weight, which is composed chiefly of fine sand, with a minute quantity of alumine and iron. The solution, which is colourless, gives with ammonia a light, flocculent, semitransparent, yellowish white precipitate, in which was recognized the presence of iron, a little alumine, and phosphate of lime. It likewise contained some traces of silex.

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* Journal des Mines, No. 119, p. 382. The whole of the paper, of which this an abridgment, will be found in the Memoirs of the National Institute.

That at Pesme.

The spar of Pesme is compact, of a grayish white, and dissolves in nitric acid, leaving a residuum of about a twentieth of its weight. A little iron, alumine, and phosphate of lime, were observed in the solution.

Almost wholly calcareous, but that of Pesme most pure.

From these two analyses it appears, that the fluors analysed consist almost wholly of calcareous matter, but that of Pesme is much the most pure. They show at the same time, that the stones examined contain a small quantity of phosphate of lime, which certainly does not amount to a five hundredth part.

II. *Analysis of the scoriæ of the iron works at Drambon.*

Scoriæ of Drambon.

Mr. Vauquelin begins with a chemical examination of these scoriæ, rather than with that of the ores and smeltings, because these scoriæ include more foreign matters in a smaller bulk.

Physical characters.

They have a shining blackish colour, nearly resembling certain oxides of manganese. Their weight indicates, that a considerable quantity of metallic matter is left in them. Some parts exhibit blebs of different sizes, others are compact. Their fracture is crystallized, either needly or laminar.

Analysed.

Five grammes [77 grains] of scoriæ, fused twice in succession with an equal weight of caustic potash, communicated to the alkali a very deep green colour, when the mass had been washed with water.

Manganese.

This green colour is known to be an unequivocal proof of the presence of manganese, and it is the best method we can employ, to discover the slightest trace of this metal in any substance.

This separated.

All the washings of the scoriæ thus treated were added together, and boiled, to separate the manganese. In proportion as this effect took place, the liquor lost its green colour, and the metal floated in it in the form of brown flocks, which, when collected, washed, and dried, weighed 2 decig. [3 grs.] amounting to 4 per cent.

Chrome suspected.

The alkaline liquor, freed from the manganese and filtered, still retained an orange yellow colour, which led Mr. Vauquelin to suspect the presence of chrome.

Silex and alumine first separated.

For verifying this suspicion, it was necessary, in order to facilitate the operations necessary for detecting the chrome, to separate the alumine and silex, that were in the alkaline lixivium; and to avoid the presence of muriatic acid, which would have thwarted the end he proposed, Mr. Vauquelin employed very pure nitrate of ammonia, instead of the muriate. Thus he obtained 2 cent. [0·3 gr.] of a mixture of silex and alumine.

Carbonic acid expelled by nitric & boiling.

He then saturated the liquor with very pure nitric acid, added a little in excess, and boiled it for a quarter of an hour, in order to dissipate the carbonic acid entirely.

Nitrate of mercury precipitated phosphoric acid.

To a portion of the liquor thus prepared he added a few drops of the solution of nitrate of mercury at a minimum; but instead of these giving it a red colour, as is usual with chrome, they threw down a white precipitate, which at first he took for muriate of mercury, but it afterward appeared to be phosphate of mercury.

Limewater threw down more.

Instructed by this trial, he added to the remainder of the liquor limewater, which, when the acid was saturated, produced a flocculent precipitate. This had a slight tint of yellow, which changed to a green on drying, a circumstance that indicated some foreign matter in the phosphate of lime.

Chrome.

Desirous of discovering the cause of this colour, he heated the precipitate red hot in a silver crucible; in consequence of which the green tint, instead of disappearing, became more intense. He then fused a little with borax by the blowpipe, and the fine emerald green colour the salt assumed confirmed his first suspicion of the existence of chrome in the scoriæ from the refining furnace.

Oxide of chrome with a little silex.

The remainder of the precipitate, being treated with nitric acid, was not entirely dissolved; a portion being left of a very deep green colour, which was nothing but oxide of chrome mixed with a little silex, the particles of which being brought together and hardened by the heat, it had lost the capacity of being soluble.

Lime.

The solution was void of colour; and oxalate of ammonia threw down from it a granulous precipitate, which when washed and dried weighed 2 decig. [3 grs.], and was true oxalate of lime.

Phosphoric acid.

The liquor from which the oxalate of lime had been precipitated, as has just been mentioned, being evaporated to dryness, and the residuum calcined, yielded an acid, which had all the properties of the phosphoric.

Chrome.

The first liquor, to which the limewater had been added to precipitate the phosphoric acid, was mixed with nitrate of mercury recently prepared; when a brown yellow precipitate was formed, which assumed a green tinge by drying in the air. The precipitate fused with borax gave it a very fine green colour, which proved it to be a chromate of mercury with excess of oxide.

All these must have existed in the pig iron and in the ore.

Thus the presence both of chrome and phosphoric acid in the scoriæ from the refining furnace is demonstrated. These matters, as well as those that will be mentioned below, existed in the pig iron, and previously in the ore, for nothing was added during the processes of working them, from which these could have been produced.

Muriatic acid oxigenized by the ferruginous part.

After the chrome, phosphoric acid, manganese, and a portion of the silex and alumine, had been separated, Mr. Vauquelin dissolved in muriatic acid the ferruginous part, which had then a yellowish red colour. He observed, that, notwithstanding the alkali had taken from it a great deal of oxide of manganese, a perceptible portion of oxigenized muriatic acid was produced, as the dissolution went on.

Silex.

A white powder remained at the bottom of the liquor, which when washed and dried weighed 88 cent. [13·6 gr.], or about a fifth of the weight of the scoriæ. During the evaporation of the liquor, which was carried to dryness, a portion of the same substance was precipitated, which was freed by means of muriatic acid from a little iron, that fell down with it. This contained some traces of chrome, for it communicated to borax a plain green colour. It was silex.

Lime.

Mr. Vauquelin precipitated the iron from its solution by ammonia, and added to the filtered solution oxalate of ammonia, which formed in it a pretty copious precipitate, that was oxalate of lime.

Manganese, alumine, and lime.

The iron, while still moist and in an attenuated state, was treated with acetous acid, the mixture evaporated to dryness, and the residuum redissolved in water. In the clear and colourless liquor were detected by different means the presence of oxide of manganese, and of alumine, which had escaped the action of the alkali in the first operation, and of a pretty large quantity of lime, which the volatile alkali had precipitated with the help of the oxide of iron.

Component parts of the scoriæ.

From these experiments, and the results they furnished, it is evident, that the dross or scoriæ of the refining furnace, on which they were made, are formed of, 1st, a large quantity of iron oxided at a minimum; 2d, oxide of manganese; 3d, phosphate of iron; 4th, chrome, probably in the state of oxide; 5th, silex; 6th, alumine; 7th, lime, part of which is perhaps combined with phosphoric acid.

All these were in the pig iron.

It can scarcely be doubted, that all these matters were contained, at least in part, in the pig iron that furnished the scoriæ: the charcoal might have imparted to them at most some lime, silex, and manganese; but the analysis of the ores, and of the pig iron itself, will soon instruct us what we ought to think on this point.

Ores examined.

III. *Examination of the bog ores.*

Bog ores of Drambon,

Chamfont, and Grosbois.

The ores subjected to analysis by Mr. Vauquelin were, 1st, those employed at the forge of Drambon. These are in spherical nodules of different sizes, and some irregular fragments of limestone are observed among them. 2d, those of Chamfont and Grosbois. These much resemble the former. Those of Grosbois contain a pretty large quantity of limestone. 3d, that of Chatillon-sur-Seine. This is of an ochry yellow colour, in grains as small as millet seed, and no limestone is seen among it, but it contains a pretty large quantity of clay.

Mr. Vauquelin gives at large his analysis of the ore of Drambon, observing, that the other ores include the same principles, though in different proportions; at the same time the quantities he has assigned to its different component parts he gives only as approximations.

Quantities only approximations.

Ten grammes [154·5 grs.] of the ore of Drambon, treated with caustic potash, assumed a very intense green colour, that communicated itself to the water in which it was lixiviated. The ore being subjected to the same operation a second time, it produced a similar effect, but less striking.

Ore of Drambon.

The liquors were boiled, and 3 decig. [4·6 grs.] of manganese fell down, containing a little silex, and an atom of iron.

Manganese, silex, and iron.

The solution retained a slight yellow colour, as in that from the scoriæ; and Mr. Vauquelin, supposing this colour to be produced by the same substance, saturated it with nitric acid. With this liquor he mixed a solution of nitrate of mercury made without heat; when it became colourless, and a white precipitate fell down, that did not give any tinge to glass of borax.

As the liquor contained an excess of acid, it was suspected, that, if any chromate of mercury had been formed, it was held in solution. Accordingly a few drops of a solution of pure potash were added, and a brown red precipitate was obtained, which, being fused with borax, gave it a fine emerald green. This indicated, that it was chromate of mercury, perhaps with a little phosphate of the same metal.

Chrome,

and perhaps phosphoric acid.

The liquor being still acid, and retaining some mercury in solution, Mr. Vauquelin imagined it still contained chrome. He therefore added a few drops of nitrate of silver, in hopes of obtaining a crimson red precipitate; but what fell down was of an orange yellow, and did not give a green colour to borax. It was phosphate of silver. Potash added to the remaining liquor produced a very bulky, flocculent, lemon-coloured precipitate. This acquired a green hue as it dried, and was chromate of mercury, containing silver, with a small quantity of alumine and silex.

Phosphoric acid.

Chrome, alumine, & silex.

The mercury was separated from the silver in a gentle heat by means of muriatic acid, diluted with two parts of water, that it might not dissolve the muriate of silver. At once the precipitate became white, and the acid green. The

Chrome.

olution

solution being evaporated to dryness left a blackish matter, which gave a very fine green colour to borax.

Magnesia.

On treating afterward with sulphuric acid, and precipitating by limewater, Mr. Vauquelin obtained 1·5 per cent of magnesia. Though this earth was found in the pig iron from each of the five bog ores, he does not venture to assert, that it exists in all: but he observes he has much more reason to think, that chrome and phosphoric acid are constantly found in it.

Similarity of these ores to meteoric stones:

Reflecting, that oxide of manganese, chrome, and magnesia, which he had just obtained, were found likewise in aerolites, or meteoric stones, he questioned whether it were not possible for iron ores, to have contributed in some way or other to the formation of these stones. This idea led him to examine, whether nickel likewise did not occur in bog ores; but his researches were fruitless.

but no nickel in them.

Component parts of the bog ores.

From what has been said it follows, that the bog ores analysed were composed of, 1st, iron; 2d, manganese; 3d, phosphoric acid; 4th, chrome; 5th, magnesia; 6th, silex; 7th, alumine; and 8th, lime. The chrome, phosphoric acid, and magnesia, had not before been noticed in these ores.

IV. *Examination of the iron, that sublimes and collects in the chimneys of the refining furnace.*

Iron sublimed into the chimneys of the furnaces,

This iron is found adhering to the sides of the chimneys of the refining furnace in the shape of stalactites, which are sometimes more than a foot long and three or four inches in diameter. They are formed of agglutinated grains, red in their fracture, leaving great intervals between them, and having but a slight action on the magnet.

We shall not give the particulars of Mr. Vauquelin's analysis, but he concludes it with the following words.

contains oxide of manganese, silex, phosphoric acid, and much chrome.

"In this sublimed iron then, there are oxide of manganese, silex, phosphoric acid, and above all a great deal of chrome. These matters therefore have been volatilised by the caloric, either by being dissolved in this fluid, or by yielding to the impulse of the current of air; but in either case they have issued from the pig iron, during the process of refining."

V.

V. *Examination of the pig iron of Drambon.*

Pig iron of Drambon.

Mr. Vauquelin having found oxide of manganese, chrome, phosphoric acid, and earths, in the scoriæ of the refining furnace, it was natural for him to infer, that he should find the same substances in the pig iron; since it is this, that furnishes these scoriæ, at least for the most part, in the process of refining. This fact was fully confirmed by analysis.

Analysed.

He proceeded in the following way. Ten gram. [154·5 grs.] of gray pig iron of Drambon reduced to filings were dissolved in sulphuric acid diluted with six parts of water. The hidrogen gas evolved during the solution was collected. It had an extremely fetid smell, very much resembling that of rotten garlic; but still more that of phosphuretted hidrogen gas, though it had a certain pungency, which the phosphuretted hidrogen has not. The nature of this gas will be noticed presently.

A very fetid hidrogen gas.

Residuum.

The residuum was of a very deep black, and diffused an extremely strong smell of phosphorus. It weighed 53 cent. [8·2 grs.] or a little more than a twentieth of the iron employed.

Oil formed.

The upper part of the bottle in which the solution was made, and the tube through which the hidrogen had passed, being so greasy that water would not adhere to them, Mr. Vauquelin suspected, that oil had been formed; a fact first announced by Mr. Proust a few years ago on a similar occasion, and which Mr. Vauquelin adds he had himself observed before that, when dissolving certain kinds of tin.

Residuum treated with hot alcohol,

To know whether any of this oil remained in the residuum of the pig iron dissolved in the sulphuric acid, he boiled it with highly dephlegmated alcohol, and filtered the liquor hot.

more oil obtained.

This alcohol became milky on the addition of water; and being exposed to a gentle heat, drops of oil separated from it as the alcohol evaporated. This oil was clear and transparent; it had a slight yellow tinge; and its taste was hot and a little pungent. It appeared to be of a middle kind between the volatile and fat oils.

After

The residuum deflagrated with nitre.

After the oil it contained was separated from the residuum of the pig iron, this residuum was deflagrated in a silver crucible with a little very pure nitrate of potash, the matter was washed with distilled water, and a light yellow liquor was obtained. This was mixed with a solution of the nitrate of ammonia, to precipitate the silex and alumine presumed to be contained in it; and a small quantity of these was separated. Limewater added to the filtered liquor formed in it a copious precipitate, which had all the characters of phosphate of lime.

Silex, alumine, and phosphoric acid separated.

Chrome.

To ascertain whether there were any chrome in this liquor, it was first boiled to volatilise the ammonia, and a few drops of nitrate of mercury were added, which was precipitated of a brown yellow, in consequence of a little lime remaining. This precipitate however gave a green colour to borax, which proves, that it contained chrome.

Lixivium.

The lixivium from the residuum of the solution calcined with nitrate of potash then contained phosphoric acid, chrome, and silex mixed with a little alumine. There was likewise in it an atom of manganese.

Residuum treated with muriatic acid.

The residuum having been thus treated and lixiviated was in the form of a reddish powder, which was dissolved for the greater part by muriatic acid. There remained however a small quantity of grayish matter, which was silex mingled with chrome, for it gave a very decided green colour to borax.

Iron with silex.

The muriatic solution contained a great deal of iron. It assumed the consistence of a jelly on evaporation, which demonstrates, that it contained silex; and it is probable, that a little chrome and manganese too were concealed in it.

Contents of the iron.

It appears then, that this pig iron contains, beside carburet of iron, phosphuret of iron, manganese, chrome, silex, and alumine. Next to the iron and carbon, it appeared to Mr. Vauquelin, that the phosphorus was most abundant. It is then in the residuums of the solutions of pig and bar iron that we must henceforward look for phosphorus, rather than in the solutions themselves, as has hitherto been done. Probably the neglect of examining these residuums with sufficient attention is the reason of our remaining

Probable reason of our ignorance of the causes of the badness of iron.

maining so ignorant of the causes of the bad quality of iron.

Phosphorus.

Mr. Vauquelin however admits, that there is likewise a small quantity of phosphorus converted into acid, and dissolved in the liquor, probably in the state of phosphate of iron, by means of the sulphuric acid. It appears to him, that, when the sulphuric acid is less diluted with water, a larger quantity of phosphorus dissolves in the liquor. To separate this phosphate of iron, he dilutes the solution with seven or eight parts of water, and mixes with it carbonate of potash, till almost the whole of the acid is saturated. A white precipitate is formed, more or less copious according to the kind of iron employed; and at the expiration of a few days it grows yellowish. This precipitate, washed and dried, he treats with potash in a silver crucible at a red heat: he then lixiviates the matter with water, and, after having saturated the liquor with nitric acid, and boiled it to expel the carbonic acid, he adds limewater, which commonly forms a white flocculent precipitate, or semitransparent if phosphoric acid be present.

Separation of the phosphate of iron.

Chrome oxigenized and dissolved in sulphuric acid.

He has likewise found a large quantity of chrome in the precipitate produced by carbonate of potash in the solution of pig iron by sulphuric acid. It follows therefore, that chrome as well as phosphorus is oxigenized and dissolved in sulphuric acid.

Caution.

It is advisable to test the alkaline liquor with nitrate of ammonia, previous to saturating it, in order to know whether it hold any silex or alumine in solution. If it do, a sufficient quantity should be added to precipitate these earths, after which they must be separated by the filter: for without this precaution they would be precipitated by the lime, and might be mistaken for phosphate of lime. Mr. Vauquelin has found very evident traces of this salt in the pig iron of the works at Drambon, though he employed sulphuric acid diluted with six parts of water to dissolve it: there was much less however, than remained in the residuum of the solution. This was the only kind of pig iron he examined, but he conceives it probable, that all the irons from bog ores contain the same foreign matters.

VI.

VI. *Examination of the bar iron of Drambon and Pesmes.*

Cold short iron analysed.

Hidrogen gas.

Mr. Vauquelin dissolved 5 gram. [77·2 grs.] of cold short iron of Drambon in sulphuric acid diluted with five parts of water. He collected the hidrogen gas, that was evolved during the dissolution, and found it to have exactly the same smell as that of the gas from the pig iron, but not quite so powerful.

Residuum.

The residuum left by these 5 gram. was much less copious than that of the pig iron, and appeared likewise not to be of so deep a black. While it was wet it emitted a very strong fetid smell, analogous to that of hidrogen gas. It weighed 15 cent. [2·3 grs.], amounting to 3 per cent. The solution of the iron had the same smell, which was not dissipated but by evaporation.

Phosphorus.

A few particles of this residuum, thrown on a burning coal, emitted a white vapour, with a smell resembling that of arsenic and phosphorus. Heated red hot in a silver crucible, it burned with flame, and left behind a yellowish powder. This was mixed with a little caustic potash, calcined, and lixiviated. The liquor being filtered, saturated with nitric acid, and subjected for a few minutes to heat, limewater was added, which threw down a white flocculent precipitate, consisting chiefly of phosphate of lime, but with an atom of silex and perhaps of alumine.

It is certain from these experiments, which Mr. Vauquelin repeated several times, that the iron of Drambon, though it is considered as of pretty good quality, contains very perceptible traces of phosphorus. He likewise found some slight traces of it in the solution by sulphuric acid.

Iron of Pesmes of better quality.

The iron of Pesmes afforded nearly the same results. The residuum however was less by one half, amounting only to 1½ per cent; and it contained less phosphorus. This iron is very tough, and is reckoned one of the best in Franche-Comté.

VII. *Of the hidrogen gas.*

The fetid hidrogen gas.

Various experiments, which Mr. Vauquelin made by the help of oxigenized muriatic acid on the hidrogen gas evolved from the pig and bar iron, led him to conclude, that phosphorus is the chief cause of its fetid smell.

VIII. *Recapitulation and inferences.*

General conclusions.

From the experiments I have related, says Mr. Vauquelin, it follows:

1. That the five sorts of bog ore I analysed are composed of the same principles, which are, beside iron, silex, alumine, lime, oxide of manganese, phosphoric acid, magnesia, and chromic acid.

2. That the five sorts of ore having been taken at a venture from places tolerably distant from each other, it is probable, that all ores of the same kind contain the same substances.

3. That these ores want only nickel, to contain the same substances, as the stones that have fallen from the atmosphere.

4. That part of these substances remains in the pig iron, and probably in larger quantity in cast iron, which may be the cause of its greater hardness and brittleness.

5. That the greater part of these substances is separated during the refining of the pig iron, when this operation is well executed; since they are found in the scoriæ, and in the sublimed iron that adheres to the insides of the chimneys of the refining furnaces.

6. That traces of them however are found in bar iron of good quality; and that probably chrome, phosphorus, and manganese, are the chief causes, that render iron hot short or cold short.

The quality of iron depends on its refining.

7. That the process of refining merits the greatest attention from iron-masters; since it appears, that the good quality of iron depends on its skilful execution.

8. That the presence of phosphorus and of chrome is to be sought for not in the solutions of pig and bar iron alone, but also in the residuums of their solutions.

9. That by the union of hidrogen and carbon during the dissolution of iron, and particularly of gray cast iron, an oil is formed, which, in conjunction with a small quantity of phosphorus, communicates a fetid smell to the hidrogen gas that dissolves them.

10. That it is to these two substances the hidrogen gas owes its properties of burning with a blue flame, and being heavier than when pure.

11. Lastly, That the oil and the phosphorus are separated from the hidrogen gas by oxigenized muriatic acid, which destroys them.

VI.

On the Maddering of Cotton and Linen Thread, and Dyeing them Adrianople Red and other fixed Colours; and on Spontaneous Inflammations: by JOHN MICHAEL HAUSSMANN*.

Fixing colours on thread.

IN order to proceed to the dyeing of cotton and linen thread all sorts of fixed colours, nothing is necessary, but to fix on the thread, in any manner whatever, more or less alumine, after having given it a slight coating of oil. The complete success of the result however depends on certain modifications to be observed in the processes.

Oils do not remain mixed with alkaline solution of alumine so well in large quantities.

The various experiments I had made in the art of dyeing had rendered me so familiar with trials on a small scale, that at length I found none of them fail. It is not till since my paper on maddering was published in the Annales de Chimie, that I experienced difficulties in the application of oils, when operating in a larger way. The linseed oil, which had always afforded me a milky mixture in limited proportions with the alkaline solution of alumine, then speedily separated, when I was desirous of making a pretty large stock, and the impregnation of the skeins became impracticable under these circumstances. It was the same with all the other fat oils: fish oil, indeed, continues mixed a pretty long time, but its smell is too offensive.

Fish oil best.

Drying oils tryed with success.

To remedy the inconvenience of the separation of the oil in the alkaline solution of alumine, I had recourse to drying oils, or those boiled with metallic oxides. Linseed oil, boiled with ceruse, minium, or litharge, by means of water to prevent its combustion, dissolves a good portion of oxide of lead, and continues mixed with the alkaline solution of alumine in a milky form, as long as is necessary for the impregnation of the skeins. If this mixture be used in the proportions and manner pointed out in my memoir, and following strictly in every other respect the process as I have described it, fine and permanent colours cannot fail to be obtained. However, notwithstanding the simplicity of the process, I can no longer recommend its use, because it has exposed me to the danger of a fire, and I will relate in what way.

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* Annales de Chimie, vol. XLVIII, p. 233.

But dangerous.

Cotton thus impregnated took fire spontaneously.

In order to see whether red cotton, which was not sufficiently fixed, might be rendered so by impregnating it with a mixture of an alkaline solution of alumine and boiled linseed oil, containing an excess of the oil, drying it, and then boiling it a very long while in bran water, I mixed the alkaline solution of alumine in the proportion of an eighth, a twelfth, and a sixteenth of boiled linseed oil. With this mixture I impregnated a few hanks of dyed cotton, which, after being left to dry a whole summer's day in the open air, were laid on a rush bottomed chair, that stood in the window of my closet. Finding myself indisposed that day, I went to bed at seven o'clock. My children went into my closet for some papers, an hour after I had left it, and perceived no heat or smell in the cotton, to indicate a commencement of burning. All the workmen had gone to bed, and were fast asleep, when one of the watchmen of the bleaching ground, seeing a great light in my closet, gave the alarm of fire, and roused us all between twelve and one o'clock. My sons, knowing that I was not able to get out of bed, and unwilling to lose time in searching for the key, broke open the door of the closet, which was in a detached, uninhabited building. They went in, notwithstanding the thick smoke and insupportable smell of the oily combustion; and found the chair with the cotton burning so furiously, that the flames rose to the ceiling, and had already cracked the glass, and set fire to the window-frame. They at once presumed, that this commencement of a fire could proceed only from the spontaneous inflammation of the cotton impregnated with boiled oil, since no one ever went into the closet with a lighted pipe, or any thing else burning.

This not believed by many.

Tried again.

As I found, that several persons belonging to the manufactory did not credit this explanation, I again impregnated a few dozen hanks of some old cotton, that had not been well dyed, in the same manner as I had done the cotton that was burned. These I set to dry in a simi'ar manner in the open air; and as it threatened to rain, ordered them to be hung upon a line under a penthouse, directing one of the watchmen to look at it every quarter of an hour during the night, and throw it into a bucket of water, as soon as he perceived it begin to heat. But this man could not believe the possibility of the cotton's taking fire of itself, as he afterward confessed to me, and walked through the manufactory without once looking at the penthouse. At length however he returned to lie down, and found by the great light he saw, that what I had foretold in case he was negligent had taken place. Finding the cotton as well as the line was burned, he took the bucket of water to extinguish the posts, which were already on fire.

Took fire as before.

Experiments on spontaneous combustion.

Though these two accidents did not at all surprise me, I could the less forgive myself for the first, as, in order to prevent similar accidents, I had made some experiments on spontaneous combustions at a public house fifteen years before. On that occasion I had spoken of the probability of fires being occasioned by heated substances, or substances that have a tendency to heat, and which are thoughtlessly put in places capable of being set on fire. The substances I mentioned to those of the company, who were not sufficiently acquainted with the phenomena of spontaneous combustion, were roasted coffee and chocolate nuts; fermented plants; ointments made with metallic oxides put hot into wooden barrels; bales of raw cotton, as well as woollen yarn or cloth packed up warm, and even linen when ironed, and put away in drawers yet hot; and lastly substances of every kind impregnated with boiling oil, as silk or cotton. I showed them besides, that in all circumstances where the oxigen of the atmosphere is rapidly attracted and absorbed by any cause, the caloric or heat, which serves as a base to the oxigen, and gives it the properties of a gas, is given out :—

Substances liable to it.

Owing to rapid attraction of oxigen.

in such abundance, that, if the absorbing substance be capable of taking fire, or surrounded by inflammable matters, spontaneous combustion will take place.

Experiments in confirmation of this.

To confirm what I had said of the theory of these sorts of combustions to those present, who were not familiar with chemical operations, I performed the following experiments. 1. The inflammation of a mixture of sulphur and iron filings kneaded with water. 2. That of boiled linseed oil by highly concentrated nitric acid. 3. That of phosphorus by atmospheric air, as well as in pure oxigen gas, placed for this purpose on a china saucer over boiling water, in order to separate its particles by fusion without having recourse to rubbing it. 4. That of phosphuretted hidrogen gas by the contact of the atmosphere, an imitation of the Jack with a lantern. 5. The combustion of pyrophorus, thrown into the open air, and into pure oxigen gas. 6. The reduction of roasted bran, put hot into a coarse bag, to an ignited coally mass by the action of the atmospheric air.

Attraction of oxigen by oils.

That in the cotton supposed to be saturated with it.

Owing in part perhaps to the proportion of oil employed.

Precaution.

I was not ignorant, that essential or volatile oils become resinous, and that drying oils boiled with metallic oxides grow thick and even hard by their combination with oxigen; and this was the reason why my hanks of cotton impregnated with a mixture of boiled linseed oil were exposed a whole day to the air, hung separately on poles: but I supposed they were then saturated with oxigen, and consequently incapable of occasioning the least accident. I felt myself so secure in this respect, that I have several times dried a great deal of oiled cotton in hot rooms; and it was owing to chance alone, that it was never put together, till the moment when it was washed in order to be dyed. It is possible however, that the proportion of a thirty-third part of boiled linseed oil mixed with the alkaline solution of alumine might be insufficient, to excite spontaneous combustion in the hanks put together after being dried. If therefore a person were inclined to employ a mixture of boiled linseed oil and the alkaline solution of alumine, on account of the simplicity of the process, he should take the precaution, to let the hanks remain spread separately on the poles, till the instant of their being washed previous to dyeing; which, in conjunction with the brightening, would remove all the excess of oil, leaving none but what was completely saturated with oxigen, and then there would be nothing to fear.

Simplest brightening for Adrianople red.

Since the publication of my memoir, I have likewise satisfied myself, that the simplest brightening for Adrianople red, by which the brightest and most lasting colours are procured, consists merely in boiling for a very long while with bran-water in a covered boiler, with a tube in the middle of the cover, to let out the steam, and prevent the bursting of the vessel. Care muſt be taken however, to change the water as often as it grows red, which will be two or three times in the beginning of the boiling; otherwise the thread would be continually taking up the dun particles, which the bran-water had removed, and a bright colour could not be obtained.

Process without danger.

All danger indeed might be avoided, without much deviation from the simplicity of my process, whether the banks were heaped up or not. Nothing more is necessary for this, but to give it a coat of olive oil in a very attenuate state, at two different times, after having well steeped it in an alkaline lixivium, washed, and dried it. For this purpose a lixivium of the subcarbonate of potash or of soda is to be made of the strength of 1° or 1½° on the saltpetre areometer. This must be tried, by mixing with it a few drops of olive oil, to see whether these produce a milky mixture, or rise and float uncombined on the top; for as the alkali may contain more or less foreign matter, the lixivium must be weakened, or strengthened by an addition of alkali, as it is absolutely necessary, that it should assume a milky appearance on the trial with oil. When the lixivium is of a proper strength, thirty-two parts are to be mixed with one of olive oil, at first by little and little, and afterward more quickly, stirring it continually the whole time. This milky mixture keeps pretty long, and if the oil begin to rise to the top in the form of cream, the mixture must be stirred afresh. The impregnation of the thread ought to be entrusted to workmen who are most expert in this process, because the accurate distribution of the oily parts has great influence on the evenness of the colour. Each workman should take only a sufficient quantity of the milky mixture in any kind of vessel, so as to be able easily to work it with all possible dexterity as many hanks as he can wring out with facility. Thus he will go on, taking constantly the same number of hanks, and the same quantity of milky mixture. What he wrings out he will put into a separate vessel, and restore to it by his eye the quantity of oil the thread has absorbed, if the trifling value of this residuum, which will contain but little oil, do not induce him to throw it away. The impregnation may be performed in the whole quantity of milky mixture, but then the quantity of olive oil, that the hanks have absorbed, will continually require to be replaced by the eye, as soon as the intensity of the milkiness appears to be diminished: the art of doing this however is easily acquired by practice. After having dried all the hanks together, they are to be impregnated a second time, in the same manner as before, but without washing them first: and when they have been again dried, they may be impregnated without previous washing, once, twice, or three times, with the pure alkaline solution of alumine without oil, in the manner described in my memoir. When they come to be dyed the colour will be more or less deep, in proportion to the number of impregnations.

For light tints.

To obtain light tints however, and at the same time even, it is better to impregnate them three times, weakening the alkaline solution of alumine proportionally. The thread might also be impregnated with this solution, either strong or weak, three times following, without previous washing; which would greatly diminish the number of operations, that are certainly tedious and troublesome: but in this case the solution must be examined from time to time, to see whether what the impregnated and dried thread discharges into it do not render it too strong.

Redyeing reds.

On redyeing red colours, it must be recollected, whether they were brightened by boiling in bran-water, or by means of soap and alkalis. In the first case they grow deeper by attracting the colouring particles of the madder; in the second they are weakened, and lose their excess of alumine, without which repeated dyeing produces no effect. The removal of this excess of alumine may be prevented, by substituting for soap and alkalis, in order to produce crimson tints, a portion of the alkaline solution of alumine, which is to be added to the bran-water toward the end of the brightening. The

The Real Adrianople reds redyed are browned by lie.

true Adrianople reds become much deeper on dyeing again, and are then browned by the test of boiling in a lixivium of wood-ashes. Before they are redyed, this changes them very little. In general reds are browned to more or less disadvantage, in proportion to the longer or shorter time they have been boiled in the brightening.

Turks use fish oil.

As the real Adrianople reds have a strong smell, the Turks perhaps use fish oil, which they add directly to the alkaline solution of alumine, or mix with a very weak lixivium of alkaline carbonate.

Process admits of great variations.

The processes for Adrianople reds may be infinitely varied; for in whatever manner, and by whatever acid or alkaline solvents the alumine is fixed in the thread, after having given it a slight coating of any kind of oil, we cannot fail to obtain reds more or less bright, in proportion to the care employed in maddering and brightening.

Oils mix with a weak solution of alkaline carbonate, not with a strong.

The reason why the oils, which very easily combine with caustic alkalis and form soap, do not mix with concentrated lixivia of alkaline carbonates, while with the same lixivia greatly diluted they form a kind of artificial milk, appears to be the more difficult to explain, as we might at least suppose, that there is a tendency to combination in these milky mixtures. A simple suspension of the integrant particles of the oil, that should take place in the diluted lixivium preferably to the stronger, is not more explicable.

Mistake corrected.

It remains for me to apologize for a misstatement I had made with regard to the fabrication of the true Adrianople red cotton used in the manufactories. What was shown to me was of very inferior quality; but I have since seen some of the finest and most permanent dye: hence I conclude, that the manufacture of the Turks, like that of all other nations, is according to the price the purchaser will give for it.

Query whether soda tend to produce the spontaneous combustion, or alumine to prevent it.

I must not omit to observe likewise, that among the cotton I had burned, there was some both times, that had been impregnated with the mixture of weak lixivium of carbonate of soda and boiled linseed oil in the proportions of an eighth, a twelfth, and a sixteenth part. It remains to be proved, whether this cotton will take fire sooner than that, which is impregnated with a mixture of the alkaline solution of alumine and boiled linseed oil in the same proportions.

As this last mixture is capable of attracting in some degree the humidity of the air, I should rather think, that the cotton treated with the first is more liable to take fire*.

In galling perhaps gallate of alumine formed, and after decomposed by the alkalis.

The experiments I have continued to make on the use of galls, in the manufacture of Adrianople red, lead me to believe, that the alumine is fixed in the cotton in consequence of the formation of a gallate of alumine, from which the gallic acid is afterward attracted by an alkaline carbonate previous to the dyeing. As soon as I have satisfied myself of the truth of this supposition, I will not fail to publish an account of my experiments.

VII.

Account of Inventions for equalizing the long and ſhort Arcs of Vibration in Timekeepers; by Mr. WILLIAM HARDY, *No. 29, Coldbath Square*†.

Equalization of arcs of vibration effected three ways.

THE equalization of the time of different arcs of vibration of the balance of a time keeper having lately given rise to much discussion, I beg leave to offer for the approbation of the Society three different modes of obtaining this end.

1st method.

The first method is by a straight spring placed edgewise across the diameter of the impellent pallet *a*, Pl. II, fig. 2 and 3, and screwed at the end opposite to the direction of the wheel, on its approach toward the centre of this pallet; at the other extremity of this spring is a flat face, or curved surface, to receive the approaching tooth of the escape wheel, which gives the impulse; this spring acts between two pins placed in the pallet near its end. By reducing this spring to a certain degree of strength, so that it may yield a little to the force of the wheel in giving the impulse, the different vibrations will be performed in the same time; but the proper degree of strength can only be determined by repeated trials. This method possesses besides this further advantage, that the acting surfaces are not so liable to be in-

Advantage.

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* Perhaps not, for this very reason. T.

† Transact. of the Society of Arts, vol. XXV, p. 113. The silver medal of the Society was voted to Mr. Hardy for this invention.

jured by the drop of the wheel upon the spring, as upon a solid surface, nor the vibrations of the balance so much disturbed by the impulse.

2d method.

The second method is by a straight spring *b c*, fig. 1 and 4, screwed to the under patt of the cock, placed edgewise and diametrically over the cylindrical spring, and having a piece cut out to clear the arbor of the balance. This straight spring is at one extremity fastened to the end of the pendulum-spring, and, at the other extremity, its elasticity is reduced so as to yield a little before the pendulum-spring operates. On the opposite of the cock, where the spring is screwed, is fixed a stud *d* projecting downward, and having a slit to admit the small piece at the end of the spring *b*. On each side of this slit is an adjusting screw *e e*, the points of which face each other, and are placed so as that the spring may move equally between them from its point of rest. The action of the spring between the adjusting screws requires to be somewhat less than the angle of escapement. Let the balance be made to vibrate, so that the straight spring may move up to the adjusting screws upon each side, and no farther; being weaker than the pendulum-spring, its exertion will be less; hence the time of the vibrations will be prolonged, but as they increase, the exertion of the pendulum-spring will commence and progressively accelerate them, and this acceleration will always be in proportion as the exertion of the pendulum-spring is to the action of the straight spring between the two adjusting screws. Thus it will always counteract the accelerating effect of the escape-wheel in the small arcs of vibration, so that the whole of them shall be performed in the same time.

3d. method.

The third method is by connecting a piece of short spring-wire to the pendulum-spring by a small piece *f*, fig. 5 and 6, with two holes; pinning the two springs together about half a turn from the stud of the pendulum-spring; and clamping the other end of the short spring at its natural point of rest to a sliding piece, *g*, which projects out from the pendulum-spring stud. By this manner of fastening, both springs will act together, and each will retain its natural point of rest; but by moving the sliding piece, which clamps the end of the short spring, and placing the spring a little on the strain, in opposition to each other's exertion, the point of rest of both springs will be destroyed. Thus, by producing this counteracting force in the two springs at the lowest point of vibration, the accelerating effect of the escape wheel upon the balance in the small arcs of vibration will be corrected, whereby the whole of them will vibrate in equal time.

Extract of a Letter from Captain William Brown, addressed to Mr. John Nichols, Millpond-bridge, Bermondsey.

Testimony of the performance of Mr. Hardy's chronometer.

Respecting the chronometer which I purchased from Mr. William Hardy last year, the jolting of the coach in the conveyance to Liverpool altered its rate of going to 34″ slow, which rate it continued so exactly, that in making Cape de Verd, on the coast of Africa, (the longitude of which has been correctly ascertained) in 24 days from Liverpool, and carefully measuring my distance from the Cape, I could not discover it to have deviated from the rate, say 34″ slow, one second in the whole time; and I have every reason to believe that it continued the same rate, until my misfortune, when it got immerged in sea-water, having lost my ship on a shoal five or six leagues from the Riopongas, this dangerous bank not being laid down correctly, or the latitude or longitude given in order to avoid it.

Dangerous shoal.

VIII.

Description of a Compensation Pendulum for a Clock, or Timepiece, with Experiments. By Mr. HENRY WARD, *of Blandford, in Dorsetshire*[*].

SIR,

New compensation pendulum.

HEREWITH I send you a new compensation-pendulum, which I beg you will lay before the Society of Arts for their inspection. I trust their liberality will be equal to the advantages that may be seen to result from it, together with their consideration of the pains I have bestowed in making it. It has cost me much labour, time, and expense; indeed, it has occupied almost the whole of my attention for the last nine months.

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[*] Trans. of the Society of Arts, vol. XXV, p. 116. The silver medal of the Society was voted to Mr. Ward.

If any objections should be made to it, I will endeavour to answer them, and make any further experiments required.

I am Sir,
Your obedient servant,
HENRY WARD.

Description of it.

Pl. II, fig. 7, *h h i i*, are two flat rods of iron or steel, about half an inch wide, and an eighth of an inch thick. *k k* is a rod of zinc interposed between them, and is nearly a quarter of an inch thick. The corners of the iron rods are bevilled off, that they may meet with less resistance from the air; and it likewise gives them a much lighter appearance. These three rods are kept together by means of three or four screws *l l l l*, which pass through oblong holes in the bars *h h k k*, and screw into the rod *i i*. The rod *h h* is connected to the rod *k k* by the screw *m*, which I call the adjusting screw. This screw turns in the rod *h h*, passes through the zinc rod *k k*, and screws into the iron rod *i i*. The rod *i i* has a shoulder at its upper end turned at right angles, and bears on the top of the zinc rod *k k*, and is supported by it. It is necessary to have several holes for the screw *m*, in order to adjust the compensation. See fig. 8.

Its action.

Now it is evident, that if any degree of heat or cold be applied to this compound rod, the one of zinc expands and contracts as much as the two iron ones together; the distance from the point of suspension to the centre of oscillation therefore must remain the same.

Smeaton's expansion table used.

In proportioning the length of the rods, I made use of Mr. Smeaton's table of expansion of metals, in the 48th vol. of the Philosophical Transactions: where he shows, by experiments made with a pyrometer, that the expansion of iron is to that of unhammered zinc, with the same degree of heat, as 151 to 353, and to that of zinc hammered half an inch per foot, as 151 to 373. This great expanding property of zinc renders it in theory extremely fit for the purpose of compensation in a pendulum, and I was desirous of knowing if it would answer in practice, and likewise the exact proportion, that was requisite to answer the intended purpose.

Two regulators made for trial.

I made two regulators, the pendulums of which were composed of iron and zinc, as above described; with this difference, however, that one had a detached scapement of a particular construction; the zinc rod was not hammered, the ball of a lenticular form, and weighed twenty pounds, its arc of vibration nearly five degrees. The other had a simple remontoiring scapement, the zinc rod was hammered, half an inch per foot, the ball, of a spherical form, weighed forty-six pounds, and vibrated two degrees and three quarters.

Difference of expansion rather greater than Mr. Smeaton's table.

These regulators were both placed in the same room, and their cases firmly fixed to the wall; the pendulums were suspended from a stout brass cock, screwed to the back of their respective cases. In the inside of each case, and immediately behind the pendulum rod, was hung a thermometer, for the purpose of comparing the degrees of heat. I adjusted them to mean time nearly by corresponding altitudes of the sun. After having compared them together for several days, I found, that the one which had the hammered zinc rod went somewhat faster when the air of the room was heated by a fire in the grate than the other did. Hence I concluded, that the difference of expansion of hammered and unhammered zinc was greater than Mr. Smeaton made it, at least it appeared so in this instance.

Contrivance for heating the air round the hammered zinc rod.

But to determine whether the length of the hammered zinc rod was accurately proportioned to that of the iron ones, without being obliged to wait that length of time that nature would require to produce a sufficient alteration in the temperature of the air, I proceeded to make the following experiment: I caused to be made a tin tube six feet long, and two inches and a half diameter at its larger end, from which it gradually tapered to the other, which was only half an inch diameter. Within the case, and as far from the pendulum as possible, I placed this tube; the smaller end was carried through a hole in the top of the case, and projected a few inches above it. In the lower end of the tube was inserted the nozzle of a lamp, and immediately under it, in the bottom of the case, was a hole of an inch diameter to supply the lamp with air. By this means the tube would communicate as much heat to the internal air, as to raise the thermometer about thirty-five degrees.

Previous to the lamp being put into the case, I made both pendulums vibrate exactly together, and after an interval of twenty-four hours, the one with the hammered zinc rod had gained, as near as I could judge, one tenth of a second. The mean height of the thermometer was fifty-three degrees. I now lighted the lamp, and in about four hours every part appeared to be thoroughly heated, and the thermometer arrived at its maximum, which was eighty-eight degrees; at this point it continued with little variation. While the beat was increasing I found the motion of the pendulum was accelerated. I again made them beat exactly together, and in about ten hours after, the heated pendulum had gained one second; the thermometer in the other case continuing nearly the same. The lamp was then taken out, and as soon as the parts were cooled, and both thermometers showed the same degree, I adjusted the beat of the pendulums as before, and, at the end of twenty-four hours, I found the pendulum that had been heated kept precisely the same rate as it did before the experiment was made.

The motion accelerated.

The pendulum adjusted afresh.

By this experiment the zinc rod was evidently too long, and that by a considerable quantity. The pendulum was then taken down, to have more holes made for the adjusting screw, and after many repeated trials with the lamp and tube, as before, I found the length of the zinc rod to be 22 inches, and consequently the length of the iron ones together 39·2 + 22 = 61·2 inches, or, the expansion and contraction of iron to that of zinc hammered half an inch per foot, as 151 to 420.

Ratio of expansion between iron and hammered zinc.

When the air was rarified the arc of vibration was less.

Having thus far satisfied myself with the hammered zinc rod, I proceeded to make similar trials with the one that was unhammered; in doing which a circumstance occurred, that I cannot account for; when the air in the case was rarified by means of the lamp and tube, the arc of vibration would be about half a degree less than it was before the lamp was applied, which is directly contrary to what I should expect would have taken place. I afterwards found, that the other pendulum was affected the same way, but in an extreme small degree, which, without doubt, was in consequence of the ball being much heavier, and vibrating a smaller arc. In taking the rate of the clock when the lamp was in the case, I at first computed from theory the errour that would arise by such a diminution of the arc, and allowed for it accordingly; but doubting whether the unlocking of the swing wheel might not form a decrease of velocity in the pendulum, and have a greater tendency to retard its motion, I therefore thought the experiment would be rendered more accurate, if the maintaining power was increased until the arc of vibration should be the same. After several trials, I found the length of the unhammered zinc rod to be about twenty-nine inches, which agrees pretty nearly with Mr. Smeaton's experiments; that is, in regard to the relative expansion of iron and unhammered zinc.

Mr. Smeaton's ratio between iron and unhammered zinc near the truth.

The zinc rod of the pendulum, which I here send to the Society of Arts, was hammered three quarters of an inch per foot; and by making experiments with it as I had done with the other two, I found the length of it to be twenty-two inches, which is exactly the same length as the one that was hammered half an inch per foot, so that it seems nothing is gained after hammering it to a certain degree; but I cannot think, that any rule can be laid down to enable us to judge of the degree of expansion that will take place with a determinate increase of heat, from the quantity that is extended by the hammer; much depends on the degree of curvature and polish of the stake and hammer, and probably on the heating of the rod at the time; for it is necessary to heat it a little hotter than boiling water, otherwise it will crack in hammering.

Farther hammering the zinc makes no alteration.

Quantity of extension by the hammer no rule.

In all these experiments it is to be understood, that the ball of the pendulum was suspended by its centre; but if the ball be made to rest on its lower edge, the

Ball suspended by its centre.

expansion and contraction of it must be taken into consideration.

Supposed objection to zinc unfounded.

It has been the opinion of some mechanists, that zinc is an unfit substance for a compensation-pendulum, because they have thought it too soft for the purpose, and that after being heated or cooled to a considerable degree, it does not return to its original dimensions. If that was really the case, no doubt but it would be a general one common to all metals in a greater or less degree; but from the experiments and observations I have made on zinc pendulums, I am fully satisfied there is no foundation whatever for such an opinion. Some time in the latter part of last summer, I however noticed a circumstance, that made me doubt the matter—for when I first used my zinc pendulum, I never could bring the clock to keep the same rate two days together, but it was continually retarded, whether I used the lamp or not; and had I not before observed a similar effect on a lever pendulum, that was made of brass and steel, I should have ascribed the cause wholly to the softness of the zinc rod; but by constantly comparing its daily rate with one that had been going a longer time, I found this retarding property gradually wore off, and in less than a month would become quite settled to the rate that it would afterwards keep. By subsequent experiments with the lamp too, I have constantly found, that all the pendulums I have hitherto tried kept precisely the same rate, both during the time they were heated (provided they were properly adjusted) and afterwards, as they had done before. The cause of this retardation appears to me to be, that the points of contact of the different pieces, which compose the pendulum, are more closely connected after a little time, than they are at first; that is, those points of contact do, by the weight of the ball, yield to each other in a small degree, until they get a broader bearing.

Pendulum at first continually retarded.

This common to others.

Owing to the effect of the ball on the point of contact.

Advantages of this pendulum.

The advantages of this pendulum are, 1st, that from its simplicity it will never fail to have the desired effect. 2dly, That no extraordinary care is requisite in executing it. 3dly, That the compensation may be increased or diminished with the greatest ease, without stopping the clock more than a minute, by making fast one of the screws that keep the rods together while the adjusting screw is removing, taking care to release it again afterwards; and 4thly, That it can be manufactured for less expense than any other compensation pendulum hitherto published.

N. B. The compensation of this pendulum which I now send to the Society of Arts is properly adjusted, at least very near the truth. The holes for the adjusting screw are made at such a distance from each other, that by removing the screw one hole, it will produce an alteration in the going of the clock of about a quarter of a second per day with a change of thirty degrees of Fahrenheit's thermometer.

SIR,

PERMIT me to state to you the observations I have made since my compensation-pendulum was laid before the Society.

Pendulum with the ball resting on the lower extremity.

The regulator, with the hammered zinc rod, and ball of forty-six pounds weight, was firmly fixed to a brick wall at the top of my house. The adjustment of the length of the rods, by means of a lamp, was repeated as before. There was, however, an alteration necessary to be noticed; the ball of the pendulum rested on its lower extremity, instead of being suspended by its centre. I prefer this method, as being less liable to errour, if the rods should be affected by heat or cold quicker than the ball. The length of the zinc rod, as ascertained by the lamp, was now found to be 20¼ inches.

Length of zinc.

The clock was then set to mean time, and suffered to go without alteration; the result is exhibited in the following table.

Rate of going.

1806		Errour of Clock at time of observation.	Number of Days between the Observations.	Daily rate.
March	21	0″ 0	18	+ 0″· 15
April	8	+ 2· 8	32	+ 0. 18
May	10	+ 8· 7	16	+ 0· 80
	26	+ 21· 5	26	+ 1· 10
June	21	+ 50· 0		

Increased

Length of zinc increased.

Increased the compensation for heat and cold, 6 holes =4½ inches, or, the length of the zinc rod to 25 inches. The clock was again set to mean time.

Rate of going.

July. 1	0″ .0	26	— 0″ 36
27	— 9· 3	13	— 0· 21
Aug. 9	— 12· 0	7	— 0· 31
16	— 14· 2	28	— 0· 34
Sep. 13	— 24· 0	12	— 0· 30
25	— 35· 5	22	— 0· 34
Oct. 17	— 52· 1		

Although a thermometer was attached to the clock, I could not from a necessary attendance to business register it regularly; the difference of its height in March and June may be taken at about 22 degrees, and in July and October 14, without much errour.

Proper length of zinc.

On comparing it with the rate of the clock, the compensation, in the latter case, appears nearly as much too great, as it was in the first too small. The true length of the zinc rod ought to be about 23 inches.

The length of the zinc rod, thus ascertained, is 1¾ inch more than the experiment by the lamp makes it; indeed, I have always suspected there might be some errour in that experiment, on account of the length of the arc of vibration being affected by it.

Having no means of finding the time accurately but by equal altitudes, I could not get so many observations as might be wished. I trust, however, these will not be found altogether useless.

I am Sir,

Your obedient servant,

HENRY WARD.

IX.

r Hardy's correction of Vibration in Time Keepers.

g. 1.

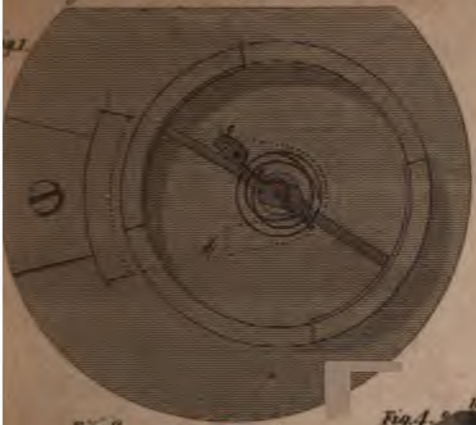


Fig. 2.

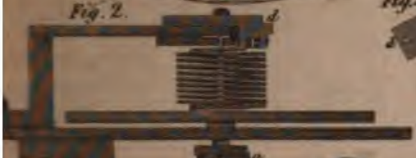


Fig. 3

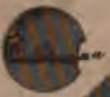
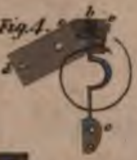


Fig. 4.



g. 5.

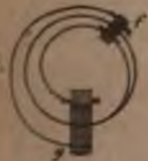


Fig. 6.

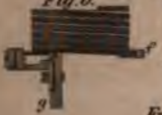


Fig. 9.

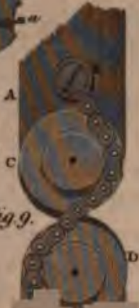


Fig. 11.

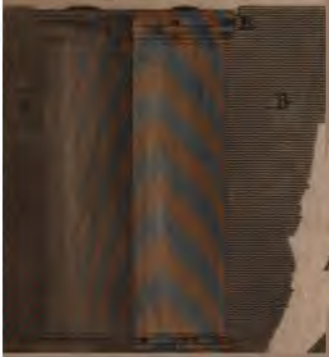


Fig. 10.

Mr. Furniss's
r Tight Door Hinge.

Fig. 7.

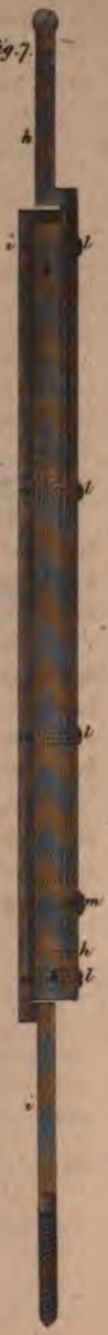
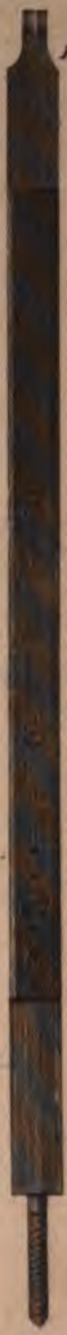


Fig. 8.



Mr. Henry Ward's Compensation Pendulum.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS

IX.

Account of a new airtight Hinge for a folding Screen, or for a Door; by Mr. MARTIN FURNISS, *No.* 128, *Strand*[*].

SIR,

THE model I have herewith sent is my invention. I beg leave to lay it before the Society for the encouragement of Arts, Manufactures, and Commerce, in the hope, that they will be pleased to examine it, and find it worthy of some mark of their approbation. It is a model for putting together the joints of a folding screen, so as to fold in either direction without admitting the smallest quantity of air; it may likewise be appropriated to hanging of doors.

Joint for a folding screen, or door, to exclude air.

I am, Sir
Your humble servant,
M. FURNISS.

A Certificate from Messrs. Wilsons, cabinet-makers in the Strand, testified, that Mr. Furniss's model for screens or doors is his own entire invention, and has been executed by them on a high folding screen for a lady in Baker street, Portman square.

Has been tried.

Reference to the Engraving of Mr. Furniss's Airtight Hinge for a Door or Screen.

Pl. II, fig. 9. A plan of the joint: A B, two sides of the screen with circular ends, joined by a piece of leather reaching from top to bottom fastened at C, and wrapping (like the letter S) partly round the curve of one fold of the screen, and partly round the other to D, where it is also

Description of the joint.

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[*] Trans. of the Society of Arts, vol. XXV, p. 126. Ten guineas were voted to Mr. Furniss for this invention.

fastened: E F, a chain formed of brass plates rivetted together, winding round in a groove from off one fold of the screen on to the other, the contrary way to the leather, so as mutually to keep each other stretched tight, the chain winding on when the leather winds off, and vice versa; thus they move smoothly round one another. G, fig. 10, a piece of brass (left out in the last figure in order to show the chain) screwed to the centre of each curve of the screens which forms the hinge, and by keeping the folds of the screen at their proper distance secures the easy action of the chains and leather, and prevents their being overstretched. H H, a line of green twist fastened along the bottom of the screen, and passing through a staple on the joint at G, serving to keep the screen airtight on the floor.

Fig. 11 is an elevation showing the top and bottom joints, with the same letters of reference.

REMARK.

It would frequently be a desirable convenience to have the doors in the interior parts of a house so contrived, as to open either inwardly or outwardly. Mr. Furniss's hinge would effectually answer this purpose: but it would be proper to have the opposite edge of the door padded, as it could not be made to fit tight, and there must be no ledge for it to abut against. A piece of leather nailed on it, and then stuffed with wool or horse-hair, might be so adapted, as to make this side airtight also, at the same time that it would open and shut freely. It is probable however, that some inconvenience might be felt in applying Mr. Furniss's hinge to a door, particularly if large and thick, from the strain upon it by the weight occasioning it to drag. The best remedy for this would be a couple of castors in the foot of the door, one near each end. C.

X.

Description of an exhausting Machine on the Principle of the Torricellian Vacuum: by Dr. THOMAS STEWART TRAILL.

Chief defect of an air pump its vacuum being imperfect.

SOME time ago I was engaged in a series of pneumatic experiments with the air pump, which led me to consider of the best means of obtaining a vacuum. The chief imperfection of an air pump consists in its not being capable of affording a *perfect* vacuum. Each stroke of the piston removes a portion of air in the receiver; but the remaining air expands, until it occupies the same volume which the whole of the included air did. The next stroke abstracts an equal volume of air with the former, but as it is now less dense, the real quantity is smaller; and hence every succeeding stroke removes a less quantity of air than the preceding. The exhaustion goes on, till the elasticity of what remains in the receiver is no longer able to open the valves of the machine, when it has reached the utmost limit. But even if the machine was constructed in the most perfect manner possible, it would evidently be impossible to obtain a complete vacuum on the principle of the air pump: for its effect is expressed by a fraction, the value of which, though constantly increasing, never amounts to unity: *i. e.* though continually approaching to, it never can afford a complete vacuum.

Attempts to apply the Torricellian vacuum.

Impressed with these objections to the air pump, it occurred to me, that, if there was a convenient method of using the Torricellian vacuum, it would be preferable to the common air pump, even when best constructed. After various attempts, the annexed figure and description will give an idea of the machine, which I conceive well adapted to answer the end proposed.

Apparatus for this purpose described.

The object in this machine is to procure a vacuum in the receiver D, by means of mercury, with which the receiver is previously filled. A (Pl. I, fig. 1) is a circular plate of thick glass, firmly imbedded in the wooden frame C, which is supported by the wooden pillars H. The surface of the plate is to be ground perfectly flat. B is an iron tube, cemented at its upper extremity in a hole drilled in the plate A, and having its lower extremity terminated at the bottom of the wooden tub E, by a stop cock, which is opened and shut by the wire F. The length of the tube ought to be about three feet; its diameter about the size of a common barometer tube. The receiver is to be ground in the usual manner to the plate, and to be fitted with an air-tight cover, I, ground to its upper orifice. K is a stop cock, through which the external air may be admitted when required. A slight depression ought to be made in the glass plate about one inch around G, that the mercury may more readily descend through the tube. The iron tube ought to reach through the glass plate to the bottom of the slight depression; and its inside at the top is to be furnished with a female screw, by which the transferrer, or any other apparatus to be used within the receiver, may be fixed to the plate. It is hardly necessary to observe, that the piece of iron which screws into the upper end of the tube B must be so perforated, as to permit the easy descent of the mercury. The inside of the tub E ought to be coated with strong varnish, to prevent the loss of mercury through its joinings, and may have a cover so fitted to it, as to keep out dust, though not to exclude air. The lower extremity of the tube B ought for steadiness to be fixed to the bottom of the tub, by a flanch and screws. The edges of C ought to project about two inches above the glass plate, that any mercury which falls over may not be lost.

The transferrer, fig. 2, is made like the plate A, and frame C, fig. 1. The lower rim of *a* is intended to rest upon the edges of C, when the iron screw *b* is fixed in G. The key of the stop-cock, *d*, passes through the lower rim, as in the figure.

Method of using the machine.

To use the exhausting machine, draw off the mercury which is in the tub E, by the stop-cock L, leaving just as much as will cover the extremity of the iron tube. Shut the stop-cock at M, pour in mercury at G to fill the tube, anoint the glass plate with hog's lard, place on the receiver, fill the receiver likewise with mercury, and then place its cover ceiver I on it. On opening M, the mercury will descend by the tube, and leave the receiver exhausted. By shutting the cock N, the vacuum is rendered more secure.

Contrivances to adapt it to different experiments, as on fluids.

A small degree of contrivance will adapt this apparatus to every experiment, which can be performed with the common air pump. If we wish to experiment on fluids, they may be enclosed in a flask of the form of fig. 3. The screw at its bottom must be perforated so as to permit the descent of the mercury when it is fixed in G. A ground stopper of glass, *e*, is to be placed in the neck of the flask, *c*, after it is filled with the liquid to be subjected to experiment: the flask is to be screwed to the plate A, and when the receiver is exhausted, the stopper is to be withdrawn by the sliding wire, fig. 4, which with its ground plate is to be substituted for I. The length of the stopper of the flask will afford room for the expansion of the liquid.

Pressure of the atmosphere shown.

When we wish to exhibit the pressure of the atmosphere by means of the apparatus, fig. 5, fill both jars, and exhaust them; force down the flat button, which is screwed on the end of the sliding wire, till it covers the orifice of the small jar, and then let the atmospheric air into the outer receiver, by the cock K. The small receiver will adhere to the plate.

By similar slight changes in the other usual apparatus of an air pump, they may be adapted to the exhausting machine.

Advantages of the apparatus described.

The advantages of an apparatus, such as I have now described, over the air pump, seem to me of considerable consequence.

1. The vacuum will be much more perfect; being only affected by the small quantity of air adhering to the mercury, or by the conversion of the mercury into vapour, which is as much as possible obviated by the cock N.

2. There will be a great saving of manual labour.

3. Exhaustion will be more quickly performed than by the common pump.

4. It is more simple than the pump, and less liable to be deranged.

Not expensive.

5. The expense of this machine will not exceed, I apprehend, that of one of the best air pumps, while it exhausts more perfectly. Where nice chemical experiments are con-

ducted, a large supply of mercury is indispensable, for there is scarcely a gas, which is not more or less absorbable by water. The mercury of the exhausting engine will answer for the mercurial trough in the laboratory, and thus a considerable portion of the expense of the machine ought to be deducted.

Trial of one rudely executed.

In some experiments I made with a rude machine of the kind I have described, I found, by anointing the plate and edge of the receiver with hog's lard, that I could raise a column of two feet in height in a receiver open at top, and even could move it along on the surface of the ground plate, without any mercury running out between the plate and receiver. In fitting on the top of the receiver, it may however be proper to press gently with the hand on the receiver, till the atmospheric pressure begins to act on it.

Materials.

N. B. The whole machine, and its auxiliary apparatus, must be made of glass, wood, and iron or steel, on which mercury does not act.

THO. STEWART TRAILL.

Liverpool, May 12, 1808.

Annotation, in Reply to the Doctor's private Letter.

Though mercury has been used for exhaustion by Dr. Clare, and Sir A. N. Edelcrantz, in air pumps described in our Journal, and by others, I have thought Dr. Traill's contrivance sufficiently original, and different from former apparatus, to be inserted.

W. N.

XI.

Doubts respecting some of the received Doctrines of Chance. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR, *Durham, August* 9, 1808.

Some received doctrines of

SHOULD the following scruples as to the truth of the elementary doctrines of chance generally admitted appear

Dr. Traill's Mercurial Exhausting Machine.

Fig. 1.

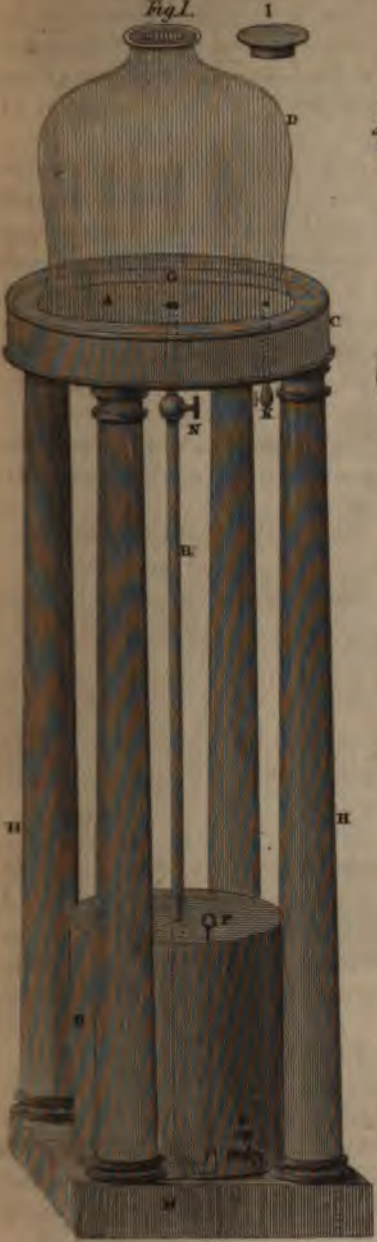


Fig. 2.

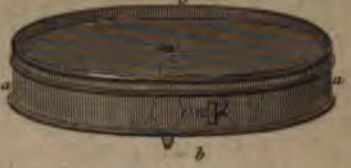
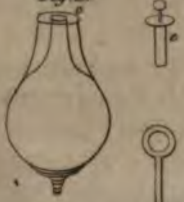


Fig. 3.



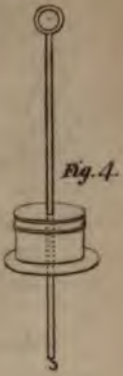


Fig. 4.

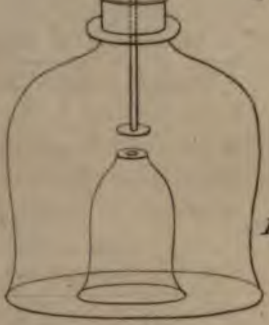


Fig. 5

Problem by J. Gough Esq.r

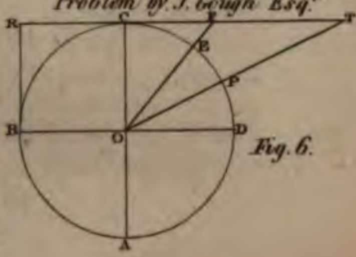


Fig. 6.

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TILDEN FOUNDATIONS
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worthy of a place in your valuable publication, their insertion will, I hope, elicit from yourself, or some one of your mathematical readers, a few words in reply, to convince or to confute a mind on all occasions suspicious of its own deductions, whenever these deviate from the opinions of others, old and learned in their walks of science.

chance questionable.

The celebrated de Moivre, in his work, Case the first, assumes the die as a familiar and favourable subject for demonstration: let us follow him in sense, though without the advantage of his identical words, as I have not a copy at hand.

"Any one undertaking, with a die of 6 sides, to cast an ace in one throw, has $\frac{1}{6}$ of the 6 possible chances in his favour, and the remaining $\frac{5}{6}$ against him; the whole 6 chances being certainty, or at least such in the event of continued trials." *Granted*—

Chance of a throw with a single die.

"Any one undertaking to cast an ace in two throws of one die, has for the first probability $\frac{1}{6}$, as proved: should the first fail, then the second remains, which is $\frac{1}{6}$ likewise; but the chance of the first failing is $\frac{5}{6}$, as that of its succeeding is $\frac{1}{6}$; therefore the second throw is only $\frac{1}{6}$ of $\frac{5}{6}$ for its chance of success, which added to the chance of casting an ace the first throw, is $\frac{1}{6}+\frac{1}{6}$ of $\frac{5}{6}=\frac{1}{6}+\frac{5}{36}=\frac{6}{36}+\frac{5}{36}=\frac{11}{36}$; the first throw being $\frac{6}{36}$, the second only $\frac{5}{36}$."

Chance of two throws.

2d chance 1 less than 2.

This doctrine I cannot grant. Nothing can prevent him of the second throw, except his succeeding in the first; therefore, either he has no occasion for it, or he has it in all its full force and virtue of $\frac{1}{6}$ chance, from which no circumstance can deduct. Otherwise it must be denied, that two equal chances are twice as good as one; and by summing up, according to de Moivre's rules, the probability of casting an ace in six throws of one die, it will of course be found, that they are below $\frac{6}{6}$, to which they should of course amount, being the assumed sum of certainty on the event, upon an average of trials.

But all the parts must be equal to the whole.

The chance of throwing a head with a halfpenny in two throws, according to de Moivre, is, for the first, $\frac{1}{2}$; for the second, only $\frac{1}{2}$ of $\frac{1}{2}$: so that the one is twice as good as the other, and together they are $\frac{1}{4}$ short in probability of what was assumed certainty, or the amount of all chances.

Instance in the toss of a halfpenny.

Any of your correspondents, or yourself, being kind enough to explain de Moivre to conviction, or my opinion to confutation, will, for I pursue but the truth, equally oblige, Sir,

Your constant reader,
and most obedient servant,
OPSIMATH.

XII.

Letter from a Correspondent on the late Discovery of Metals in the fixed Alkalis.

SIR,

Assertion of Dr Beddoes, that the earths and alkalis contained oxigen.

YOUR last reminds me of some notes I took at Oxford, on attending Dr. Beddoes's lectures in 1788, wherein he said, that vital air was a part of the alkalis and earths. At the same lectures, the strongest electricity was advised, for giving shocks to molten phosphorus, &c. Some years ago, at a friend's, I saw a book with essays by several hands—Dr. Beddoes and Mr. Davy among others. It is called Contributions, &c. I think;—be that as it may, a query is started, whether the earths and alkalis hold not oxigen, and that they may come to class with metals. It is always curious to know who has guessed best. If you have the above book, you may find reasons for opinions then seeming strange, and farther particulars.

Their metallic nature conjectured.

A. DILETTANTE.

8*th Aug.* 1808.

REMARK.

Metals long ago supposed to be obtained from earths.

The notion of alkalis being oxigenated metals capable of being reduced, is much older than the book in question. The experiments of von Ruprecht and Tondi at Schemnitz, with the discussions which arose from them among some of the most eminent chemists, not only in Germany, but in Italy and France, are fresh in remembrance. It is certain, that metals more or less resembling iron, or phosphuret of iron, were produced, in appearance from barytes, lime, magnesia,

nesia, and *borax*; and though this was at length supposed to be refuted, as similar grains of metal were obtained without either of these, yet I believe in this case an alkali was always present. It must be confessed however, the metal produced by the German chemists was extremely different from either of the metallic bases of the alkalis lately discovered by Mr. Davy.

An alkali always present.

XIII.

On the Decomposition of the Alkalis. In a Letter from Mr. William Cooke.

To Mr. NICHOLSON.

SIR, *Wolverhampton*, 10*th July*, 1808.

In your excellent Journal for January last, under the head Scientific News, is announced the decomposition of the alkalis, by that eminent chemist, Professor Davy; whose very name almost deters one from entertaining a contrary idea: but the conclusions drawn by him do not appear to me to arise from the facts adduced. Since that time I have turned over your Journal, and other publications, in hopes that some one with more leisure and abilities would have pointed out, not [illegible] want of accuracy in his experiments, but of clearness in the conclusions drawn therefrom; but not seeing any thing of this kind, I have determined to devote a few minutes from business to offer the following: and if it appear worthy of your notice, it is much at your service, from,

Decomposition of the alkalis questioned.

Sir,

Your most obedient servant,

WILLIAM COOKE.

In Volume XIX, page 78, of your Journal, it appears, that Mr. Davy made *moistened* potash, soda, &c. part of a galvanic circle, in which situation oxigen gas was evolved, and a substance

Moistened alkali placed in a galvanic circle.

Properties of the substance produced.

a substance of a metallic appearance remained, which was denominated the base of potash, &c., and possessed, among others, the following properties. If a globule were thrown into water, or upon ice, a bright flame and great heat were produced, hidrogen gas was evolved, and the alkali found in solution; if upon moist turmeric paper, the same phenomena appeared, with the acquirement of a rapid motion, and its course marked with a red or brown stain, proving the reproduction of the alkali. In all which instances it is stated, that this metallic body has such an affinity for oxigen, that it instantly decomposes water, absorbing its oxigen (which regenerates alkali) and its hidrogen of course is set at liberty. But, if the experiments with metals be faithfully reported, alkalis are regenerated from these supposed bases, either by losing or absorbing oxigen.

Alkali regenerated by losing or absorbing oxigen.

Matter of electricity capable of combination.

It seems reasonable to conclude, that the matter of electricity is as capable of combining chemically with bodies, as the matter of heat or light is; and that Mr. Davy has found the means of uniting another of the simple combustibles with the alkalis.

Sulphuric acid a compound of caloric.

Perhaps to say *concentrated* sulphuric acid is a calorated oxigat of sulphur may appear barbarous; yet it is impossible to form it without the union of caloric, or to dilute it without the loss thereof; therefore, as I cannot find a more expressive name for these new bodies, I will call them electrated hidrats of potash, soda, &c.; wherein the hidrogen has so weak an affinity for the alkalis, that solution decomposes them; for on coming into contact with water, they are so rapidly decomposed, that the matter of electricity becomes visible, the hidrogen takes the gaseous form, and of course the alkali remains in solution.

New bodies, hidrats combined with electricity.

In chemical experiments all the component parts should be accounted for.

The importance of accounting for the *whole* of the simples submitted to chemical experiment cannot be too often enforced; and that experiments may be depended upon, it is absolutely necessary. The overlooking of this seems to me the only cause of Mr. Davy's miſtake: for, as the alkalis were moistened, and it is the known property of the galvanic fluid to decompose water, and as one of its component parts was evolved, it was absolutely necessary to inquire after the other; more especially, as the body produced was found to be incompatible with the presence of water.

Thus, if the above arguments be conclusive (but, from my time being necessarily devoted to business, I have not opportunity of submitting them to the rigorous test of experiment, as they deserve, or as I could wish), it appears, that these new substances, instead of being the bases of alkalis, are compounds of alkali and hidrogen united by means of the electric fluid.

XIV.

On the Quantity of Fecula in different Varieties of the Potato. By Mr. WILLIAM SKRIMSHIRE, *Jun.*

SIR, *Wisbech, Aug.* 12, 1808.

IF the following paper, on the quantity of fecula in the different varieties of the *solanum tuberosum*, which was lately read before a small society of philosophical amateurs in this town, be deemed worthy a place in your valuable miscellany, I shall be glad to have it inserted: and shall soon follow this up by a second communication, on the quantity of fecula procured from some other vegetables of British growth, and the economical purposes, to which they may be applied.

Philosophical society at Wisbech.

I remain, yours, &c.

W. SKRIMSHIRE, JUN.

In the early part of the present summer I undertook a series of experiments, to ascertain the quantity of fecula contained in the several varieties of the potato cultivated in this neighbourhood, which I take the liberty of laying before the society, for their information, and as a subject well worthy of a farther investigation.

Quantity of fecula in the potato sought.

But as the following experiments were made with the fresh roots, and at a time of the year when most of them were in a growing state, the several results can be viewed merely

Experiment made with fresh roots.

merely as approximations toward the truth, or at most, as showing the relative quantity of fecula, afforded by the different varieties which were operated upon.

Cautions.

When experiments are made in the large way, with the fresh potato, the different degrees of moisture, which the roots may possess, will materially influence the results; so will the form of the grater, and the force which is employed in grating them. Therefore when great accuracy is required, the potatoes should be sliced, dried, and ground to meal, before being subjected to experiment.

Fecula most abundant in autumn, partly changing to saccharine matter in spring.

Perhaps the greatest quantity of fecula may be procured in the autumn, as soon as the potatoes are dug up; for those that have been preserved through the winter are so disposed to vegetate in the spring, that they then contain more of the saccharine matter than they do in the autumn: and this is produced at the expense of the fecula, for it is probable, that, as the fecula absorbs oxigen and hidrogen, it parts with a portion of its carbon; and is thereby converted into sugar.

Potato attracts moisture from the atmosphere.

Another circumstance, which may very much affect the apparent quantity of fecula, is its precise state of dryness when weighed; for it quickly attracts moisture from the atmosphere, and therefore should always be weighed at a certain temperature, in a dry room.

Fecula dried in a roaster.

In the following experiments, the fecula, after being dried in the open air, was placed for some hours in a Rumford roaster, moderately warm, and weighed as soon as it was taken out. This perhaps is one reason why my produce is generally below that of Dr. Pearson, which he communicated to the Board of Agriculture, as well as by his not using the skin of the potatoes in his experiments, as I did in those which follow.

Skin of the potato included.

Dr. Pearson's experiments with the kidney potato.

From Dr. Pearson's account we learn, that 3500 grains of fresh potato root, commonly called the white kidney, being dried, leave 1000 grains:

That 100 parts of the fresh root, deprived of skin, afford

1. Water	68 to	72
2. Meal	32 to	28
	100	100

The meal consists of three substances,

1. Starch or fecula	17 to 15
2. Fibrous matter	9 to 8
3. Extract or soluble mucilage	6 to 5
	32 .. 28

Thus 100 parts of fresh root afford from 15 to 17 of fine dry fecula*.

Method in which Mr. Skrimshire's experiments were made.

The following are the results of my experiments made with five pounds of each variety of the potato, weighed after being washed clean, brushed with a hard brush, and wiped dry with a clean linen cloth. The root was afterward grated in cold water. The whole of the pulp and water was placed in a fine hair sieve to drain, and fresh water poured over it, stirring up and squeezing it with the hand, until the water passed through perfectly clear.

By this operation *almost* the whole of the fecula or starch is washed from the fibrous matter, and falls to the bottom of the vessel in the form of a firm white precipitate. This precipitate was again edulcorated with water, and passed through a fine silk sieve, which separated it still more from the finer particles of the pulp. The fecula was then allowed to settle, and being collected was dried by a free exposure to the air, on a clean linen cloth.

1. *Captain Hart.*

Potato called captain Hart.

This is a roundish white potato, with a thin smooth skin, of a moderate size, and with but few eyes. When boiled and peeled it appears of a yellow colour; its consistence is rather close and watery, but it is tolerably well flavoured.

A peck weighs from 14 to 15 lb.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		9
Fecula slightly discoloured		3
Pulp dried		6
Water, soluble mucilage, and extractive matter	3	14
	5	0

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* Repert. Arts, &c. vol. III, p. 383.

2. *Rough Red.*

Rough red.

This is a round red potato, of a moderate size, with a thin skin, rough with minute fissures and scales. When boiled it is very mealy, but has rather a strong flavour.

A peck weighs from 13 to 14 lbs.

Five pounds weight afford,

	lb.	oz.
Fine white fecula		7¼
Fecula discoloured		3¼
Pulp dried		6½
Water, soluble mucilage, and extractive matter	3	15
	5	0

3. *White Kidney.*

White kidney.

This is a clean white potato, of a tolerable size, variously shaped, generally flattened, and often with an indentation on one edge, giving it some resemblance to a kidney in its form. The skin is rather thick; when boiled it is not very mealy, but pleasant flavoured, and a very good potato for the table.

A peck weighs from 14 to 15 lbs.

Five pounds weight afford,

	lb.	oz.
Fecula, the whole of which was of an indifferent colour*		9¼
Pulp dried slightly brown†		3¾
Water, soluble mucilage, and extractive matter	4	2¼
	5	0

4. *Moulton White.*

Moulton white.

This is an irregular shaped white potato, of a tolerable size. It is sometimes flattened like the kidney; and indeed its general appearance bears a striking resemblance to the white kidney potato. Its skin is rather thick. When boiled it is very mealy, and remarkably well tasted. It is by far the best potato for the table.

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* This I attribute to the potatoes being much grown.

† The roaster, being too hot, when the pulp was put in, it was rather scorched.

A peck weighs about 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		9
Fecula slightly discoloured		2¾
Pulp dried		5¼
Water, soluble mucilage, and extractive matter	3	14¼
	5	0

5. *Yorkshire Kidney.*

Yorkshire kidney.

This is nothing like the kidney potato, and I think was misnamed. It is a thin, long, white root, with several eyes, is very scabby, and has a thick skin. When boiled it is slightly mealy, but has a strong taste.

A peck weighs from 14 to 15 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		8¾
Fecula slightly discoloured		2¼
Pulp dried		6¼
Water, soluble mucilage, and extractive matter	3	14¼
	5	0

6. *Hundred Eyes.*

Hundred eyes.

This is a long white potato of a midling size. It has numerous eyes, with a narrow transverse depression below each eye. When boiled it has no unpleasant taste, but is rather too close in its consistence to be reckoned a good potato for the table.

A peck weighs from 14 to 15 lbs.

Five pounds weight afford,

	lb.	oz.
Fine white fecula		8¼
Discoloured fecula		¾
Pulp dried		6¾
Water, soluble mucilage, and extractive matter	4	0¼
	5	0

7. *Poor Man's Profit, or Purple Red.*

Poor man's profit, or purple red.

This is a large round purple potato, with a thin skin. When boiled it is hard and close, but has no very unpleasant taste.

A peck weighs from 15 to 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine fecula very white		8
Very brown fecula		½
Pulp dried		5
Water, soluble mucilage, and extractive matter	4	2½
	5	0

8. *Ox Noble.*

Oxnoble.

This is a very large, round, white potato; but when it grows extremely large, it is frequently found hollow. It is principally used for feeding cattle. When boiled it is close and watery, with a very strong taste.

If perfectly sound, a peck usually weighs from 15 to 16 lbs.

Five pounds weight afford,

	lb.	oz.
Fine white fecula		6½
Fecula slightly discoloured		1¾
Pulp dried		8
Water, soluble mucilage, and extractive matter	3	15¾
	5	0

XV.

XV.

Account of a Phenomenon, that occurred at Bussora. From Travels in Asia and Africa, by the late ABRAHAM PARSONS, *Esq., Consul and Factor-Marine at Scanderoon.*

Sudden darkness

MARCH the 15th, 1775. At four this afternoon, the sun then shining bright, a total darkness commenced in an instant, when a dreadful consternation seized every person in the city of Bussora, the people running backward and forward in the streets, tumbling over one another, quite distracted, while those in the houses ran out in amazement, doubting whether it were an eclipse, or the end of the world. Soon after the black cloud which had caused this total darkness, approached near the city, preceded by as loud a noise as I ever heard in the greatest storm. This was succeeded by such a violent whirlwind, mixed with dust, that no man in the streets could stand upon his legs; happy were those who could find, or had already obtained, shelter, while those who were not so fortunate were obliged to throw themselves down on the spot, where they ran great risk of being suffocated, as the wind lafted full twenty minutes, and the total darkness half an hour. The dust was so subtile, and the hurricane so furious, that every room in the British factory was covered with it, notwithstanding we had the precaution to shut the doors and windows on the first appearance of the darkness, and to light candles.

attended with a violent whirlwind mixed with dust.

At half past five the cloud had passed the city, the sun instantly shone out, no wind was to be heard, nor dust felt, but all was quite serene and calm again; when all of us in the factory went on the terrace, and observed the cloud had entirely passed over the river, and was then in Persia, where it seemed to cover full thirty miles in breadth on the land, but how far in length could not be even guessed at: it flew along at an amazing rate, yet was half an hour in passing over the city. It came from the north-west, and went straight forward to the south-east.

The cloud 30 miles broad, & half an hour in passing.

The officers of the company's cruizers came on shore as soon as the cloud had passed their ships, and declared that the wind was so violent, and the dust so penetrating, that no man could stand upon the decks; and that after it was over, every place below on board the ship was covered with dust. Such a phenomenon never was known before, in the memory of the oldest man now living at Bussora.

SCIENTIFIC NEWS.

St. Thomas's and Guy's Hospitals.

Medical and surgical lectures.

THE autumnal Course of Lectures at these adjoining Hospitals will commence the beginning of October: viz.

At St. Thomas's.

Anatomy and the Operations of Surgery, by Mr. **CLINE** and Mr. **COOPER.**

Principles and Practice of Surgery, by Mr. **COOPER.**

At Guy's.

Practice of Medicine, by Dr. **BABINGTON** and Dr. **CURRY.**

Chemistry, by Dr. **BABINGTON**, Dr. **MARCET**, and Mr. **ALLEN.**

Experimental Philosophy, by Mr. **ALLEN.**

Theory of Medicine, and Materia Medica, by Dr. **CURRY** and Dr. **CHOLMELEY.**

Midwifery, and Diseases of Women and Children, by Dr. **HAIGHTON.**

Physiology, or Laws of the Animal Œconomy, by Dr. **HAIGHTON.**

Occasional Clinical Lectures on select Medical Cases, by Dr. **BABINGTON**, Dr. **CURRY**, and Dr. **MARCET.**

Structure and Diseases of the Teeth, by Mr. **FOX.**

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete course of Medical and Chirurgical instructions.

London Hospital.

On Monday, the 3d of October, Dr. **BUXTON** commences a course of Lectures on the Theory and Practice of Medicine, and one on Materia Medica.

Chemical and mineralogical lectures.

Mr. ACCUM's Lectures on Experimental Chemistry and Analytical Mineralogy, commence at the Chemical Laboratory, Compton Street, Soho, October the 18th.

The Lectures on Experimental Chemistry comprise the Practical Operations of the Scientific Laboratory, General Rules to be observed in the performance of Experiments, and Summary Experimental Elucidations of the Science of Chemical Philosophy.

The Lectures on Analytical Mineralogy are devoted to the art of distinguishing minerals, the modes of examining them by chemical agencies and general processes of analysis; with a summary view of the nature of Mineralogical Science, and its application to the useful arts and manufactures.

Account of the Situation of the Instruments employed by Mr. Robert Banks, *for the Meteorological Journal.*

Barometer.

Calculation of its height above the sea.

THE barometer, the height of which is registered every day at 9 A. M., is placed at the height of 27 feet from the ground. Now we learn from Dr. Young, that the Thames at Buckingham stairs, 15½ feet below the pavement in the left hand arcade, is 43 feet above the level of the sea, by barometrical comparison with the Seine and the Mediterranean. But he observes, that this calculation probably gives the height too great. Mr. Brindley, levelling from Boulter's Lock to Mortlake, by order of the City of London, in the year 1770, found the fall upon 41 miles to be 75½ feet. On the last 8 miles of this distance, however, the fall was only 12 feet. Now if we allow the fall from Buckingham stairs to the Lower Hope to average only 1 foot per mile, the difference of level will be at least 35 feet. This, added to 42 feet, the height of the ground where Mr. Banks's house stands above the Thames at Buckingham stairs, and 27 feet, the height of the barometer above the ground, we shall have 104 feet for the height of the barometer above the level of the sea.

104 feet.

Thermometers.

The thermometers hang 5 feet from the ground, against a wall that has nearly an eastern aspect, and is completely sheltered from the sun both at its back and front the whole day, in such a manner that it cannot be affected by its heat, either direct or reflected. Five are generally employed for the purpose, because it is well known, if the bulbs be not of the same size, they are subject to vary a little when the temperature is taken at any stated hour, some rising or falling more quickly than others from this circumstance, though a little sooner or later they would indicate the same height. For this reason a mean of them is taken.

Weather.

Under the head of weather, if any rain have fallen during the day, the word *rain* is inserted in the day column. The weather in the night column is noted about eleven o'clock, P.M., at which time, if any rain fall, rain is set down; if it do not rain, yet no stars are to be seen, the word *cloudy* is inserted; when there is no rain, and a greater or less number of stars are visible, it is marked as *fair.*

METEOROLOGICAL JOURNAL

For *AUGUST*, 1808,

Kept by ROBERT BANKS, Mathematical Instrument Mak
in the STRAND, LONDON.

JULY. Day of	THERMOMETER.				BAROMETER.	WEATHER.	
	11 A. M.	11 P. M.	Highest.	Lowest.		Night.	Day.
26	66	64	69	61	29·82	Fair	Rain
27	67	63	72	60	29·83	Rain	Fair
28	63	62	66	62	29·52	Cloudy	Rain
29	66	63	70	62	29·68	Fair	Fair
30	68	67	73	61	29·76	Ditto	Ditto
31	70	67	72	62	29·82	Rain*	Rain
AUG.							
1	68	65	75	60	29·64	Fair	Ditto
2	69	65	78	60	29·83	Ditto	Fair
3	64	64	69	60	30·09	Ditto	Ditto
4	67	65	75	60	30·06	Ditto	Ditto
5	68	71	74	64	29·88	Ditto	Ditto
6	69	70	76	62	29·75	Ditto	Ditto
7	68	65	73	61	29·86	Ditto	Rain
8	66	65	72	61	29·82	Ditto	Ditto
9	63	65	71	60	29·64	Ditto †	Ditto ‡
10	62	64	71	62	29·78	Ditto	Fair ‡
11	61	64	70	59	29·74	Ditto	Ditto
12	66	66	73	62	29·82	Ditto	Ditto
13	64	66	68	60	29·73	Cloudy	Rain
14	67	63	72	60	29·70	Fair	Ditto
15	64	63	68	61	29·66	Cloudy	Ditto
16	65	63	69	60	29·87	Fair	Ditto
17	64	64	69	60	29·92	Cloudy	Fair
18	65	64	70	61	30·03	Fair	Ditto
19	64	64	69	58	30·10	Ditto	Ditto
20	64	65	71	58	30·15	Ditto	Ditto
21	65	66	72	62	30·17	Ditto	Ditto
22	65	64	71	58	30·16	Ditto	Ditto
23	64	62	75	59	30·15	Cloudy	Ditto
24	65	62	72	58	30·11	Fair	Ditto
25	63	63	68	56	30·11	Ditto	Ditto
26	64	62	70	59	29·86	Ditto	Ditto

* Hard rain, thunder, and vivid lightning all the evening.

† Thunder at 6 P. M. ‡ Thunder.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1808.

ARTICLE I.

Method of making a Composition for Painting in Imitation of the ancient Grecian Manner, with Remarks. By Mrs. Hooker, *of Rottingdean, near Brighton*[*].

SIR,

I Had the pleasure to communicate to the Society for the Encouragement of Arts, Manufactures, and Commerce, in 1786, when Miss E. J. Greenland, my method of painting in imitation of the ancient Grecian manner or encaustic painting; and in consequence, they did me the honour to adjudge to me the gold pallet, and also afterward to approve my account of the result of above fifty experiments per day, which I made during more than four months in 1792, in the hope of discovering some means of making wax, gum mastich, and water unite like a cream, in order to expedite the formation of the composition for imitating the encaustic painting, which was published the same year by the Society

First attempt to imitate the ancient Grecian painting.

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[*] Trans. of the Society of Arts, vol. XXV, p. 45. This lady's first account of her method of painting was published in the 10th vol. of the Trans. of the Society.

of Arts. I now take the liberty of sending them another copy, but with some alterations and many additions, which I trust will be found calculated to facilitate and improve that method of painting, as they have arisen from much observation and reflection on several pictures I have painted since I had last the honour of addressing the Society. In consequence of the application of several gentlemen of the profession, I have drawn up this paper, which, considering the former attentions of the Society, I thought it would be proper for me to offer first to them for their acceptance, but if they should not think it worthy of communication, I hope they will pardon the intrusion, and attribute it only to the sense of gratitude I feel for the honour already conferred on,

Sir,

Your most obedient servant,

EMMA JANE HOOKER.

Method of preparing and applying the composition.

Method of preparing the composition.

Put into a glazed earthen vessel four ounces and a half of gum arabic, and eight ounces, or half a pint (wine measure) of cold spring water; when the gum is dissolved, stir in seven ounces of gum mastich, which has been washed, dried, picked, and beaten fine. Set the earthen vessel containing the gum-water, and gum mastich, over a slow fire, continually stirring and beating them hard with a spoon, in order to dissolve the gum mastich: when sufficiently boiled, it will no longer appear transparent, but will become opaque, and stiff, like a paste. As soon as this is the case, and that the gum-water and mastich are quite boiling, without taking them off the fire, add five ounces of white wax, broken into small pieces, stirring and beating the different ingredients together, till the wax is perfectly melted and has boiled. Then take the composition off the fire, as boiling it longer than necessary would only harden the wax, and prevent its mixing so well afterwards with water. When the composition is taken off the fire, and in the glazed earthen vessel, it should be beaten hard, and while hot (but not boiling) mix with it by degrees a pint (wine measure) or sixteen ounces more of cold spring water, then strain the composition, as some dirt will boil out of the gum mastich, and put it into bottles: the composition, if properly made, should be like a cream, and the colours, when mixed with it, as smooth as with oil.

Method of using it.

The method of using it is, to mix with the composition, upon an earthen pallet, such colours in powder as are used in painting with oil; and such a quantity of the composition is to be mixed with the colours, as to render them of the usual consistency of oil colours; then paint with fair water. The colours when mixed with the composition may be laid on either thick or thin, as may best suit your subject; on which account, this composition is very advantageous, where any particular transparency of colouring is required; but in most cases it answers best, if the colours be laid on thick, and they require the same use of the brush, as if painting with body colours, and the same brushes as used in oil painting. The colours, if grown dry, when mixed with the composition, may be used by putting a little fair water over them; but it is less trouble to put some water when the colours are observed to be growing dry. In painting with this composition the colours blend without difficulty when wet, and even when dry the tints may easily be united by means of a brush and a very small quantity of fair water.

The colours may be moistened with water, when grown dry.

Wax to be applied to the painting.

When the painting is finished, put some white wax into a glazed earthen vessel over a slow fire, and when melted, but not boiling, with a hard brush cover the painting with the wax, and when cold take a moderately hot iron, such as is used for ironing linen, and so cold as not to hiss, if touched with any thing wet, and draw it lightly over the wax. The painting will appear as if under a cloud till the wax is perfectly cold, as well as whatever the picture is painted upon; but if, when so, the painting should not appear sufficiently clear, it may be held before the fire, so far from it as to melt the wax but slowly; or the wax may be melted by holding a hot poker at such a distance as to melt it gently, especially such parts of the picture as should not appear sufficiently transparent or brilliant; for the oftener heat is applied to the picture, the greater will be the transparency and brilliancy of colouring; but the contrary effect would be produced, if too sudden or too great a degree of heat was applied, or for too long a time, as it would draw the wax too much to the surface, and might likewise crack the paint. Should the coat of wax, put over the painting when finished, appear in any part uneven, it may be remedied by drawing a moderately hot iron over it again as before mentioned, or even by scraping the wax with a knife: and should the wax by too great or too long an application of heat form into bubbles at particular places, by applying a poker heated, or even a tobacco-pipe made hot, the bubbles would subside; or such defects may be removed by drawing any thing hard over the wax, which would close any small cavities. When the picture is cold, rub it with a fine linen cloth.

Wood, canvass, pasteboard, or plaster of Paris may be painted on thus.

Paintings may be executed in this manner upon wood (having first pieces of wood let in behind, across the grain of the wood, to prevent its warping), canvass, card, or plaster of Paris. The plaster of Paris would require no other preparation than mixing some fine plaster of Paris in powder with cold water the thickness of a cream; then put it on a looking-glass, having first made a frame of bees wax on the looking-glass the form and thickness you would wish the plaster of Paris to be of, and when dry take it off, and there will be a very smooth surface to paint upon. Wood and canvass are best covered with some gray tint mixed with the same composition of gum arabic, gum mastich, and wax, and of the same sort of colours as before mentioned, before the design is begun, in order to cover the grain of the wood or the threads of the canvass.

A composition without wax.

Paintings may also be done in the same manner with only gum-water and gum mastich, prepared the same way as the mastich and wax; but instead of putting seven ounces of mastich, and when boiling, adding five ounces of wax, mix twelve ounces of gum mastich with the gum-water, prepared as mentioned in the first part of this receipt: before it is put on the fire, and when sufficiently boiled and beaten, and a little cold, stir in by degrees twelve ounces, or three quarters of a pint (wine measure) of cold spring water, and afterward strain it.

A composition without the mastich.

It would be equally practicable painting with wax alone, dissolved in gum-water in the following manner. Take twelve ounces, or three quarters of a pint (wine measure) of cold spring water, and four ounces and a half of gum arabic, put them into a glazed earthen vessel, and when the gum is dissolved, add eight ounces of white wax. Put the earthen vessel with the gum-water and wax upon a slow fire, and stir them till the wax is dissolved, and has boiled a few minutes: then take them off the fire and throw them into a basin, as by remaining in the hot earthen vessel the wax would become rather hard; beat the gum-water and wax till quite cold. As there is but a small proportion of water in comparison to the quantity of gum and wax, it would be necessary in mixing this composition with the colours, to put also some fair water. Should the composition be so made as to occasion the ingredients to separate in the bottle, it will become equally serviceable, if shaken before used with the colours.

The composition grows dry by keeping, but may be softened afresh with water.

I had lately an opportunity of discovering, that the composition which had remained in a bottle since the year 1792, in which time it had grown dry and become as solid a substance as wax, returned to a creamlike consistence, and became again in as proper a state to mix with colours, as when it was first made, by putting a little cold water upon it, and suffering it to remain on a short time. I also lately found some of the mixture composed of only gum arabic water and gum mastich, of which I sent a specimen to the Society of Arts in 1792; it was become dry, and had much the appearance and consistency of horn. I found, on letting some cold water remain over it, that it became as fit for painting with, as when the composition was first prepared.

II.

On Oxalic Acid. *By* THOMAS THOMSON, *M. D. F. R. S. Ed.* *Communicated by* CHARLES HATCHETT, *Esq. F. R. S.*

(Concluded from p. 32.)

IV. *Composition of Oxalic Acid.*

Composition of oxalic acid.

THE knowledge of the relative weights of the elements which compose oxalic acid, though of importance, is not sufficient to convey a clear idea of this compound, and in what respect it differs from tartaric acid, alcohol, sugar, and various other bodies possessing very different properties, though composed of the very same elements in different proportions.

Elements always combine in determinate proportions which may be expressed by numbers.

It has been ascertained, by numerous and decisive experiments, that elementary bodies always enter into combinations in determinate proportions, which may be represented by numbers. For example, the numbers which correspond to the four elements, oxigen, azote, carbon, and hidrogen, are the following:

Instance.

Oxigen	6
Azote	15
Carbon	4·5
Hidrogen	1

Now, in all compounds consisting of these ingredients, the proportion of the different constituents may always be represented by these numbers, or by multiples of them; thus, the composition of the following substances may be thus stated.

Compounds of oxigen, nitrogen, carbon, and hidrogen.

	Oxigen.	Hidrogen	Carbon.	Azote.
Water	6	+1		
Carbonic oxide	6		+4·5	
Carbonic acid	2×6		+4·5	
Carburetted hydrogen		2×1	+4·5	
Olefiant gas		1	+4·5	
Nitrous gas	6			+5
Nitric acid	2×6			+5
Nitrous oxide	6			+2×5

From

These numbers probably represent the weight of their primitive atoms.

From the knowledge of this curious law, it is difficult to avoid concluding, that each of these elements consists of atoms of determinate weight, which combine according to certain fixed proportions, and that the numbers above given represent the relative weights of these atoms respectively. Thus, an atom of oxigen weighs six, an atom of hidrogen one, &c. Water is composed of one atom of oxigen, and one atom of hidrogen; carbonic acid of two atoms of oxigen, and one of carbon, &c. This curious theory, which promises to throw an unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton. I have elsewhere illustrated it at considerable length*.

The same law holds in salts.

The same law holds with respect to the salts. The acids and bases always combine in determinate proportions. We may affix numbers to all the acids and bases, which numbers, or their multiples, will represent all the combinations into which these bodies enter. Some of these numbers are given in the following table:

Sulphuric acid	33	Barytes	67
Muriatic acid	18	Soda	24
Carbonic acid	16·5	Lime	23
Nitric acid	17	Ammonia	6

These numbers may be conceived to represent the relative weights of an integrant particle of each of these substances; formed on the supposition, that an atom of hidrogen weighs 1. It follows equally from this law, that the acids and bases combine particle with particle, or a certain determinate number of particles of the one with a particle of the other.

Corollary.

Weight of integrant particles an object of importance.

One of the most important points in the investigation of compound bodies is, to ascertain the number, which denotes the weight of an integrant particle of each of them, that of an atom of hidrogen being one; because this number, or a multiple of it, represents the weight of each, which enters into all combinations; and because it enables us to estimate the number of elementary atoms, of which each is composed. From a careful comparison of the table of oxalates, given in a preceding part of this paper, with the weight of the different bases already determined*, it appears, that the weight of an integrant particle of oxalic acid must be represented by the number 39·5.

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* See System of Chemistry, III, 424, &c. 3d Edition.

Integrant particle of oxalic acid.

Now, what number of atoms of oxigen, carbon, and hidrogen, go to constitute an integrant particle of oxalic acid? We have assigned the relative weights of each of these atoms, and we have ascertained the relative proportions of the respective elements of oxalic acid. From these data it is easy to solve the problem. An integrant particle of oxalic acid consists of 9 atoms combined together, namely, 4 atoms of oxigen, 3 of carbon, and 2 of hidrogen.

4 atoms of oxigen weigh······ $4 \times 6 = 24$
3 atoms of carbon············ $3 \times 4{\cdot}5 = 13{\cdot}5$
2 atoms of hidrogen·········· $2 \times 1 = 2$

Total············ 39·5

which together make up the weight of an integrant particle of oxalic acid.

Component parts of the acid according to this.

According to these proportions, 100 parts of oxalic acid is composed of

Oxigen············	61
Carbon············	34
Hidrogen··········	5
	100

numbers which do not indeed exactly correspond with the result of the preceding analysis, but which approach sufficiently near it, to give the reasoning employed considerable probability at least, if it does not lead to certainty.

Decomposition of oxalate of lime by heat explained.

We may now examine the decomposition which takes place, when oxalate of lime is exposed to heat. Let an atom of oxigen be w, an atom of carbon c, and an atom of hidrogen, h. An integrant particle of oxalic acid may be represented by $4\,w + 3\,c + 2\,h$. We may represent the composition and weight of an integrant particle of each of the substances into which oxalic acid is decomposed by heat, by the following symbols and numbers:

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* For these weights, and the method of determining them, I refer the reader to my System of Chemistry, 3d Edition, III, 619. The numbers which I have there assigned are, I am persuaded, rather too low.

Carbonic

Carbonic acid	$2w + c$ weight	16·5
Carburetted hidrogen	$c + 2h$	6·5
Carbonic oxide	$w + c$	10·5
Water	$w + h$	7
Charcoal	c	4·5

We may now conceive 3 particles of oxalic acid to be decomposed at once, and to resolve themselves into these substances, in the following proportions:

4 particles of carbonic acid …… = $8w + 4c$
2 particles of carburetted hidrogen = $2c + 4h$
2 particles of carbonic oxide …… = $2w + 2c$
2 particles of water ………… = $2w + 2h$
1 particle of charcoal ………… = $1c$

Total ……… $12w + 9c + 6h$
3 particles of oxalic acid ……… = $12w + 9c + 6h$

We see that such a decomposition is possible. It remains only therefore to see, whether the weights of these substances, which result from this hypothesis, correspond with the preceding analysis. Now,

4 particles of carbonic acid weigh …… $4 \times 16·5 = 66$
2 ——— carburetted hidrogen … $2 \times 6·5 = 13$
2 ——— carbonic acid ………… $2 \times 10·5 = 21$
2 ——— water ……………… $2 \times 7 = 14$
1 ——— charcoal …………… $4·5 = 4·5$

Total ………………… 118·5

Reducing these proportions to 100 parts of acid, and joining together the two inflammable gasses, the numbers come out as follows:

		we actually obtained
Carbonic acid	55·70	59·53
Inflammable air	28·69	24·28
Water	11·81	11·51
Charcoal	3·80	4·68
	100·00	100·00

It is impossible to expect exact correspondence between the theory and analysis, till the numbers representing the weights

Hypothesis and facts nearly agree.

weights of the elementary atoms be ascertained with more rigid accuracy, than has hitherto been done. I satisfied myself with taking the nearest round numbers, which are sufficient at least to show an evident approximation to the proportions obtained by experiment.

V. *Composition of Sugar, and Formation of Oxalic Acid.*

Composition of sugar and formation of oxalic acid.

When a compound body is decomposed, and resolved into a number of new substances, the products are almost always simpler, or consist of integrant particles composed of fewer atoms than the integrant particles of the original body. Thus, though oxalic acid is composed of 9 atoms, none of the products evolved, when that acid is decomposed by heat, contain more than 3 atoms. Hence it is probable, that sugar is a more compound body than oxalic acid, because nitric acid resolves it into a variety of new compounds, one of which is oxalic acid. It may be worth while to examine the action of nitric acid on sugar, and the formation of oxalic acid, more closely than has hitherto been done, as the investigation will furnish some data for estimating the composition of sugar.

200 grains of sugar treated with nitric acid.

Two hundred grains of pure crystallized sugar, being treated with diluted nitric acid in the usual way, yielded 200 cubic inches of carbonic acid, 64 cubic inches of nitrous gas, and 70 cubic inches of azotic gas. But these numbers, though the result of a good many experiments, are not to be considered as very exact. The uncertainty depends upon the property, which the solution has of producing more gas after the sugar is decomposed, at the expense of the oxalic acid formed. Now it is difficult to stop at the precise point.

116 grains of oxalic acid produced.

The whole weight of oxalic acid, which can be obtained from 200 grains of sugar, amounts to 116 grains. If the experiment be properly conducted, the whole of the sugar is decomposed, or at least the quantity of residuary matter is small.

From the preceding statement, there is reason to conclude, that 100 grains of sugar, when decomposed by nitric acid, yield,

	Grains.
1. Oxalic acid crystals 58 grains, or real acid	45
2. Carbonic acid 100 cubic inches, equivalent to	46·5:

Oxalic and carbonic acids.

while there are evolved obviously, by the decomposition of the nitric acid,

	Grains.
1. Azotic gas 35 cubic inches, equivalent to	10·62
2. Nitrous gas 32 cubic inches, equivalent to	10·85

Now, as nitric acid contains no carbon, it is obvious, that the oxalic acid formed, and the carbonic acid evolved, must contain the whole carbon contained in 100 grains of sugar.

	Grains.
45 grains of oxalic acid contain of carbon	14·40
46·5 grains of carbonic acid contain of ditto	13·02
Total	27·42

therefore 100 grains of sugar contain 27½ grains of carbon.

100 sugar contain 27·5 carbon.

The azotic gas and nitrous gas must have been originally in the state of nitric acid, and must have given out oxigen when they were evolved. But nitric acid is composed of

			Oxigen.
Azote	10·62	+	25
Nitrous gas	10·85	+	4·5
			29·5

Therefore they must have parted with 29·5 grains of oxigen. We are at liberty to suppose, that the whole of this oxigen went to the formation of carbonic acid. Now, 46·5 grains of carbonic acid are composed of

	Grains.
Oxigen	38·5
Carbon	13·0
	46·5

From this it appears, that in the carbonic acid there were 4 grains of oxigen more than was furnished by the nitric acid. I confess I am disposed to ascribe this surplus to errours in the experiments, and to believe, that the whole of the oxigen of the carbonic acid was furnished by the nitric acid.

Apparent surplus of oxigen.

acid. This being admitted, it follows, that the carbon of the carbonic acid, and the whole constituents of the oxalic acid, were furnished by the sugar. These are as follows:

	Grains.
Carbon	27·5
Oxigen in 45 grains oxalic acid	28·8
Hidrogen in ditto	1·8
	58·1

Water presumed to be formed.

If this total be substracted from the 100 grains of sugar used, there will be a remainder of 41·9 grains. As this quantity of the sugar has disappeared, and is no where to be found among the products, we must suppose, that it has assumed the form of water. Now 41·9 grains of water are composed of

Oxigen	35·9
Hidrogen	6
	41·9

Adding these quantities to the preceding products, we obtain the composition of sugar, as follows:

Component parts of sugar.

Oxigen	64·7
Carbon	27·5
Hidrogen	7·8
	100·0

This cannot be relied on implicitly,

but corroborated by Lavoisier's analysis.

Though the process of reasoning, which led to this analysis of sugar, is too hypothetical to be trusted implicitly, yet I am persuaded, that it is to a certain degree correct, and that the result obtained does not deviate very far from the truth. If we compare Lavoisier's statement of the composition of sugar obtained in a different manner, though by a mode of reasoning not less hypothetical, we shall be surprised at its near coincidence with mine. His numbers are

Oxigen	64
Carbon	28
Hidrogen	8
	100

Integrant particle of sugar.

Its difference from oxalic acid.

Theory of the formation of oxalic acid from sugar.

ne, which cannot be rejected; for it is clear, that the
of oxigen, carbon, and hidrogen, will be to each
spectively, as the numbers $\frac{64}{6}$, $\frac{28}{4\cdot5}$, $\frac{8}{1}$; and these num-
uced to their lowest terms become 5, 3, 4, nearly,
being primes with respect to each other, must repre-
number of atoms, of which an integrant particle of
composed. Sugar then is a compound of 12 atoms;
five of oxigen, three of carbon, and four of hidro-
e weight of an integrant particle of it is 47·5, and
bol is $5\,w + 3\,c + 4\,h$. It differs from oxalic acid
in containing an additional atom of oxigen and two
gen. If we had any method of removing these
ces, without altering the proportion of the other con-
s, we should obtain a much greater quantity of
acid from sugar than we can at present; but nitric
s by removing one half of the carbon in the form of
c acid; the sugar, deprived of this, resolves itself
lic acid and water. Suppose two particles of sugar
at once, the symbol for them will be $10\,w + 6\,c + 8\,h$.
ee atoms of the carbon be removed by the action
nitric acid, there will remain $10\,w + 3\,c + 8\,h$.

$$\begin{array}{lcl} \text{A particle of oxalic acid} & = & 4\,w + 3\,c + 2\,h \\ \text{Six particles of water}\cdots & = & 6\,w + + 6\,h \\ \hline & & 10\,w + 3\,c + 8\,h, \end{array}$$

s just the quantity of oxalic acid left. This will cede the oxalic acid, and afterward to be converted into it by the subsequent action of nitric acid; but on the composition and formation of this latter acid I avoid making any observations at present, as I propose to make them the subject of a separate dissertation.

III.

An Account of the Application of the Gas from Coal to economical Purposes. By Mr. WILLIAM MURDOCH. *Communicated by the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S.**

Light from coal gas applied on a large scale.

THE facts and results intended to be communicated in this Paper are founded upon observations made, during the present winter, at the cotton manufactory of Messrs. Phillips and Lee at Manchester, where the light obtained by the combustion of the gas from coal is used upon a very large scale; the apparatus for its production and application having been prepared by me at the works of Messrs. Boulton, Watt, and Co. at Soho.

Quantity of light required on a comparison with candles.

The whole of the rooms of this cotton mill, which is, I believe, the most extensive in the United Kingdom, as well as its counting-houses and store-rooms, and the adjacent dwelling-house of Mr. Lee, are lighted with the gas from coal. The total quantity of light used during the hours of burning, has been ascertained, by a comparison of shadows, to be about equal to the light which 2500 mould candles of six in the pound would give; each of the candles, with which the comparison was made consuming at the rate of 4-10ths of an ounce (175 grains) of tallow per hour.

Experiment made under very favourable circumstances.

The quantity of light is necessarily liable to some variation, from the difficulty of adjusting all the flames, so as to be perfectly equal at all times; but the admirable precision and exactness, with which the business of this mill is conducted, afforded as excellent an opportunity of making the comparative trials I had in view, as is perhaps likely to be ever obtained in general practice. And the experiments being made upon so large a scale, and for a considerable period of time, may, I think, be assumed as a sufficiently accurate standard for determining the advantages to be expected from the use of the gas lights under favourable circumstances.

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* Philos. Trans. for 1808, p. 124.

Method in which the gas is obtained and applied.

It is not my intention, in the present paper, to enter into a particular description of the apparatus employed for producing the gas: but I may observe generally, that the coal is distilled in large iron retorts, which during the winter season are kept constantly at work, except during the intervals of charging; and that the gas, as it rises from them, is conveyed by iron pipes into large reservoirs, or gasometers, where it is washed and purified, previous to its being conveyed through other pipes, called mains, to the mill. These mains branch off into a variety of ramifications (forming a total length of several miles), and diminish in size, as the quantity of gas required to be passed through them becomes less. The burners, where the gas is consumed, are connected with the above mains, by short tubes, each of which is furnished with a cock to regulate the admission of the gas to each burner, and to shut it totally off when requisite. This latter operation may likewise be instantaneously performed, throughout the whole of the burners in each room, by turning a cock, with which each main is provided, near its entrance into the room.

Two kinds of burners.

The burners are of two kinds: the one is upon the principle of the Argand lamp, and resembles it in appearance; the other is a small curved tube with a conical end, having three circular apertures or perforations, of about a thirtieth of an inch in diameter, one at the point of the cone, and two lateral ones, through which the gas issues, forming three divergent jets of flame, somewhat like a fleur-de-lis. The shape and general appearance of this tube, has procured it among the workmen the name of the cockspur burner.

904 burners,

The number of burners employed in all the buildings amounts to 271 Argands, and 633 cockspurs; each of the former giving a light equal to that of four candles of the description giving light equal to 2500 mould candles of 6 to the lb. consume 1250 cub. feet of gas from cannel coal hourly.

description abovementioned; and each of the latter, a light equal to two and a quarter of the same candles; making therefore the total of the gas light a little more than equal to that of 2500 candles. When thus regulated, the whole of the above burners require an *hourly* supply of 1250 cubic feet of the gas produced from cannel coal; the superior quality and quantity of the gas produced from that material having given it a decided preference in this situation, over every other coal, notwithstanding its higher price.

This requires 3½cwt. of cannel coal in the retorts,

The time during which the gas light is used may, upon an average of the whole year, be stated at least at two hours per day of twenty-four hours. In some mills, where there is over work, it will be three hours; and in the few where night work is still continued, nearly twelve hours. But taking two hours per day as the common average throughout the year, the consumption in Messrs. Philips and Lee's mill, will be 1250 × 2 = 2506 cubic feet of gas per day; to produce which, seven hundred weight of cannel coal is required in the retort. The price of the best Wigan cannel (the sort used) is 13½*d.* per cwt. (22*s.* 6*d.* per ton), delivered at the mill, or say about eight shillings for the seven hundred weight. Multiplying by the number of working days in the year (313), the annual consumption of cannel will be 110 tons, and its cost £125.

and above 1-3d as much good common coal to heat them.

About one third of the above quantity, or say forty tons of good common coal, value ten shillings per ton, is required for fuel to heat the retorts; the annual amount of which is £20.

Produce in coak.

The 110 tons of cannel coal, when distilled, produce about 70 tons of good coak, which is sold upon the spot at 1*s.* 4*d.* per cwt. and will therefore amount annually to the sum of £93.

Tar.

The quantity of tar produced from each ton of cannel coal is from eleven to twelve ale gallons, making a total annual produce of about 1250 ale gallons, which not having been yet sold, I cannot determine its value; but whenever it comes to be manufactured in large quantities, it cannot be such as materially to influence the economical statement, unless indeed new applications of it should be discovered.

Produce trifling.

The

Aqueous fluid.

The quantity of aqueous fluid, that came over in the course of the observations which I am now giving an account of, was not exactly ascertained, from some springs having got into the reservoir; and as it has not been yet applied to any useful purpose, I may omit further notice of it in this statement.

Interest of capital.

The interest of the capital expended in the necessary apparatus and buildings, together with what is considered as an ample allowance for wear and tear, is stated by Mr. Lee at about £550 per annum: in which some allowance is made for this apparatus being upon a scale adequate to the supply of a still greater quantity of light, than he has occasion to make use of.

Attendance not more than on candles.

He is of opinion, that the cost of attendance upon candles would be as much, if not more, than upon the gas apparatus; so that in forming the comparison, nothing need be stated upon that score, on either side.

The economical statement for one year then stands thus:

Expense of the gas lights.

Cost of 110 tons of cannel coal	£125
Ditto of 40 tons of common ditto	20
	145
Deduct the value of 70 tons of coak	93
The annual expenditure in coal, after deducting the value of the coak, and without allowing any thing for the tar, is therefore	52
And the interest of capital, and wear and tear of apparatus	550

making the total expense of the gas apparatus, about £600 per annum.

That of candles.

That of candles, to give the same light, would be about £2000. For each candle consuming at the rate of 4 tenths of an ounce of tallow per hour, the 2500 candles, burning upon an average of the year two hours per day, would, at one shilling per pound, the present price, amount to nearly the sum of money abovementioned.

The comparison more favourable to the gas where the

If the comparison were made upon an average of three hours per day, the advantage would be still more in favour of the gas light; the interest of the capital, and wear and an apparatus upon a larger scale, which during many successive nights was applied to the lighting of their principal building, and various new methods were practised of washing and purifying the gas.

Illumination in 1802.

These experiments were continued with some interruptions, until the peace of 1802, when a public display of this light was made by me in the illumination of Mr. Boulton's manufactory at Soho, upon that occasion.

Since regularly used at the Soho foundry.

Since that period, I have, under the sanction of Messrs. Boulton, Watt, and Co. extended the apparatus at Soho foundry, so as to give light to all the principal shops, where it is in regular use, to the exclusion of other artificial light; but I have preferred giving the results from Messrs. Philips and Lee's apparatus, both on account of its greater extent, and the greater uniformity of the lights, which rendered the comparison with candles less difficult.

The inflammable nature of the gas long known,

At the time I commenced my experiments, I was certainly unacquainted with the circumstance of the gas from coal having been observed by others to be capable of combustion; but I am since informed, that the current of gas escaping from Lord Dundonald's tar ovens had been frequently fired; and I find that Dr. Clayton, in a paper in volume XLI of the Transactions of the Royal Society, so long ago as the year 1739, gave an account of some observations and experiments made by him, which clearly manifest his knowledge of the inflammable property of the gas, which he denominates "the spirit of coals;" but the idea of applying it as an economical substitute for oils and tallow does not appear to have occurred to this gentleman, and I believe I may, without presuming too much, claim both the first idea of applying, and the first actual application of this gas to economical purposes.

but first applied to economical purposes by Mr. Murdoch.

REMARK.

Intention of applying coal gas to light the streets of the metropolis.

As an attempt to light the streets of the metropolis by means of coal gas has made much noise, and is meant, as it is said, to be brought before parliament next session, whatever tends to elucidate the subject cannot be uninteresting. The account here given by Mr. Murdoch, whose experiments were mentioned in our Journal for June 1805, is perfectly satisfactory with respect to the application of coal gas as the material of furnishing light, and its comparative cheapness and advantages, at least in a coal country: but it must be obvious, that his calculations are by no means adapted to London.

Mr Murdoch's estimate not applicable to London.

Mr. Murdoch states the annual expense for lighting the manufactory of Messrs. Philips and Lee at £600; and observes, that, to produce an equal light by candles would cost £2000. The cannel coal employed however, as being most profitable though sold at the highest price, costs there only 22*s.* 6*d.* per ton, and the coal for heating the retorts only 10*s.* per ton. The coak produced there sells at 26*s.* 8*d.* per ton. Now on inquiry at a very respectable coal merchant's in London I find, that cannel coal sells here at £4 per ton; the coal for the furnaces may be averaged at 45*s.*, and the coak at 50*s.* It must be observed too, that the apparatus being manufactured on the spot at Birmingham, it of course was so much the less expensive. The statement for the metropolis therefore would probably stand thus.

Calculation for the metropolis for two hours:

Cost of 110 tons of cannel coal at £4 ········	£440
——— 40 tons of common coal at 45*s.*········	90
Interest of capital, and wear and tear of apparatus ································	650
	1180
Deduct for 70 tons of coak at 50*s.*············	175
	1005

Thus the expenditure would be £1005 to procure light equal to that of as many candles as would come to £2000. This is the calculation for light for two hours a day. If we take it for three hours a day, according to Mr. Murdoch's second estimate, the calculation will be

or if extended to three.

168 tons of cannel coal ·····················	£672
60 tons of common coal ·····················	135
Apparatus as before ························	650
	1457
Deduct for 105 tons of coak ················	263
	1194

or £1194 to procure the light of £3000 worth of candles.

When

For a longer time more favourable.

When the gas is applied to lighting the streets by night however, perhaps we may state the time at an average as ten hours a day; and the longer the time, as Mr. Murdoch justly observes, the greater will be the balance in favour of the gas, since the apparatus will remain nearly the same.

But various deductions must be made.

But then, there are several circumstances farther to be taken into consideration. Though the greater part of the apparatus would not be altered, it appears an increase of some parts would be necessary, if the burning were to be continued beyond three hours. We must add too to the expenditure, the rent of houses in every part of the town for holding the furnaces and apparatus, the wages of persons to attend these, and the salaries of clerks, none of which were necessary in Mr. Murdoch's case. Besides, the price of coal must probably be increased by the additional consumption, and that of coak would certainly fall very greatly, from the quantity produced beyond the demand for it. The estimate too must be made in comparison with common lamp oil, the expense of which may be reckoned at not more than two thirds the cost of candles. Farther, the gas lights are said to give double the light of the common street lamps, and this is certainly an accommodation to the public: but then, as the calculation of Mr. Murdoch is founded very properly on the quantity of light given, this will affect the estimate in a similar ratio, so that the expense of the oil must be diminished by one half. On these grounds the estimate would appear somewhat in the following form, taking it at an average of ten hours every night.

Estimate without these deductions.

550 tons of cannel coal	£2200
200 tons of common coal	450
Interest of capital, and wear and tear of apparatus	800
	3450
Deduct for 350 tons of coak	875
	2575.

The expense of lighting to an equal extent with oil, according to the estimation above given, would be £3333. It must be observed, nothing is here allowed for the coal-tar, the produce of which from the above quantity of coal, according to Mr. Murdoch, would be about 6000 ale gallons, or 7000 wine gallons; as he has left it out of his estimate from being of too trifling value. Neither have I taken into account the expense for rents of houses, salaries, and wages, with the other circumstances abovementioned, that must affect the profits; though I have said enough to show, that these, if any, must differ very widely indeed from the enormous gains held out to the public, to induce incautious individuals to embark in the project, when it was first set on foot.

For farther remarks on the coal gas lights see our Journal, vol. XVI, p. 73, 83, 308. C.

IV.

Farther Experiments on the Spleen. *By* EVERARD HOME, *Esq. F.R.S.*[*]

Object in view.

THE results of the experiments already brought forward[†] having established the fact, that fluids received into the stomach, when the pylorus is closed, pass through the spleen into the circulation of the blood; it became an object to determine, by experiment, whether this takes place, when the parts are in a natural state.

The ass a favourable subject.

The ass appeared, on many accounts, the best subject for this purpose; and as it is made use of to teach the veterinary pupils the anatomy of that tribe of animals, I applied to the Professor for permission to make my experiments in the theatre of the college.

This was granted me in the most obliging manner; the subjects were also supplied by the College, and Mr. Sewell, the assistant Professor, gave me his personal aid with a degree of zeal and ability I have rarely met with, and have much pleasure in acknowledging.

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[*] Philos. Trans. for 1808, p. 133.

[†] See our Journal, vol. XX, p. 374.

In making the following experiments, I had the assistance of Mr. Sewell, Mr. Brodie, Mr. William Brande, and Mr. Clift.

Exp. 1. Tincture of rhubarb given diluted with water.

Experiment 1. An ass, which had been kept twenty-four hours without hay, to prevent the liquor that was to be poured into its stomach from being soaked up and retained there, on the evening of the 3d of December, 1807, had a drench given it, consisting of half a pint of the spirituous tincture of rhubarb, diluted in half a pint of water. On the morning of the 4th, this was repeated at eight o'clock, and again at twelve. At two o'clock the animal was pithed, so as to destroy its sensibility; and before the circulation was entirely stopped, six ounces of blood were taken from the splenic vein into a graduated glass measure, and a similar quantity was taken from the left auricle of the heart, into a vessel of the same kind: these were allowed to coagulate and separate their serum.

The animal pithed. Blood drawn from the splenic vein and left auricle.

State of the spleen.

Rhubarb conveyed to it, but not to the liver.

The spleen was large and turgid; upon making sections of it, the cells were found to be very numerous; and towards the great end and near the edge, they were particularly distinct to the naked eye. The cut surface had a strong smell of rhubarb, and when it was applied to white paper wetted with the alkaline test, an orange tinge was produced. This was strongly contrasted by a stain made in the same manner with a section of the liver, which had no such tinge, nor did the liver give the slightest smell of rhubarb.

Rhubarb in the urine most: next in infusion of spleen, then in serum from splenic vein, and least in serum from left auricle and infusion of liver.

Infusions were made of the spleen and liver under similar circumstances; these were strained off into separate glasses, and tested by the alkali. The urine was tested in the same way. The serum, from the different portions of blood, was also poured off into separate glass vessels, to which the test was added. In nineteen hours after the blood had been taken from the veins, they were all compared together. The urine had so deep a tinge, that it nearly resembled the pure tincture of rhubarb in appearance; the others had a tinge, although in very different degrees; the quantity of rhubarb they contained was estimated by adding tincture of rhubarb to alkaline water so as to produce corresponding tints. The infusion of spleen had a tint equal to sixty drops of tincture of rhubarb in two ounces of alkaline water: the serum of the splenic vein to fifteen drops: the serum from the left auricle of the heart, to three drops. The infusion of the liver gave no orange tinge, but had it not been obscured by the red particles of the blood, it must have been equal to that of the serum from the auricle.

Connecting membrane between the stomach & spleen examined.

The connecting membrane between the stomach and spleen was attentively examined, very few absorbent vessels were seen, and these were not in a turgid state, they were traced to the chain of glands situate near the edge of the spleen, which receive the absorbents of the stomach, but none were detected passing beyond the glands, nor did the glands admit quicksilver to pass through them towards the spleen.

Exp 2. Results similar, but in a less degree; perhaps from inflammation of viscera.

Exp. 2. The former experiment was repeated upon another ass, with similar results, but less strongly marked; the cause of this difference was explained by the abdominal viscera being in an inflamed state.

The urine was less impregnated with rhubarb, the infusion of the spleen had a lighter tinge, and the serum of the splenic vein had it in a still less degree; but evidently exceeding that of the serum from the vena cava inferior opened just below the diaphragm, which was substituted for the left auricle of the heart, with a view to vary the experiment.

Exp. 3. Similar.

Exp. 3. The same experiment was made on a third ass with similar results.

Exp. 4. Powdered rhubarb given.

Exp. 4. An ass that had been kept four days without water, and two without solid food, on the evening of the 8th of January, 1808, had a ball given it, containing half an ounce of powdered rhubarb; on the 9th, at seven o'clock in the morning, this was repeated; a third was given at nine o'clock, and a fourth at twelve. At two o'clock the ass was pithed, and four ounces of blood were taken from the splenic vein, and the same quantity from the left auricle of the heart.

Spleen much contracted.

The spleen was found contracted to half the size of those in the former experiments; when cut into the cells were small, and it required a magnifying glass to see them distinctly. The substance was compact, and bore a near resemblance to a portion of liver; so that in this state the blood vessels, particularly the veins, must have been much contracted in their diameters.

Other viscera.

The stomach contained about two ounces and a half of a gelatinous substance mixed with rhubarb, the small intestines were nearly empty, but the cæcum and colon contained several quarts of water, in which the rhubarb was more evident both to the sight and smell, than in the stomach.

The absorbent glands upon the edge of the colon were ranged in two rows, one on each side of the great vein, and were exceedingly numerous. In the space between these rows of glands, in some places twenty trunks of absorbent vessels could be readily counted, of a very large size.

Rhubarb in the urine.

The urine was impregnated with rhubarb, so as to acquire an orange tinge from the addition of the test; but the infusion of the spleen, and the serum of the different portions of blood, did not contain it in sufficient quantity to have the colour heightened by alkali.

Exp. repeated with similar results.

Exp. 5. The last experiment was repeated upon another ass. Two ounces of blood were taken from the splenic vein, two from the large vein of the colon, and two from the inferior vena cava in the lower part of the loins.

The spleen had the same appearance as in the last experiment.

The stomach contained nearly a pint of moderately solid contents, in which the rhubarb was very evident. The small intestines were nearly empty; but the cæcum and beginning of the colon contained several quarts of liquid, strongly impregnated with rhubarb.

The absorbent glands and vessels had the same appearance as in the former experiment.

The urine when tested was found impregnated with rhubarb.

The proportions of serum of the blood taken from these different veins, when tested by the alkali, appeared to be very much alike; at least that from the splenic vein was not more tinged than the others.

Spirituous liquors produce inflammation

Exp. 6. Having been informed by Mr. Sewell, that spirituous liquors, given in large quantities to horses, produce inflammation of the brain in horses.

inflammation of the brain, and sometimes death; and this information having been in some measure confirmed by an ass in a weakly state, that had taken half a pint of the spirituous tincture of rhubarb in the evening, dying in the night; I thought it right to make a comparative experiment with the infusion of rhubarb, to determine whether the result would be the same as with the tincture.

Exp. 6. aqueous infusion of rhubarb given.

February 9, 1808. An ass had a pint of infusion of rhubarb given to it in the evening; the same dose was repeated at six o'clock in the morning of the 10th; and again at nine o'clock, and at twelve. At two o'clock the animal was pithed, and two ounces of blood were taken from the splenic vein, two from the vein of the colon, and two from the inferior vena cava in the lower part of the loins.

Rhubarb found in the spleen,

The spleen was found turgid, and large; when the cut surface was rubbed on white paper, the orange tint was very evident without any test applied to it, particularly so, when compared with a similar stain made by a section of the liver, in which there was no such tinge.

stomach and duodenum,

In the stomach and duodenum, the rhubarb was found in large quantities; but none was met with in the cæcum.

urine,

The urine was impregnated with rhubarb, the orange tint upon the application of the alkali being very distinct.

and serum.

At the end of twenty hours, the serum of the splenic vein had a tinge equal to four drops of the tincture of rhubarb in two ounces of alkaline water; that of the vein of the colon and vena cava was less distinct.

Effects slighter than with the tincture.

The effects of the infusion of rhubarb on the spleen, the serum of the blood, and the urine, corresponded exactly with that of the tincture in the former experiments, but was in a less degree of intensity.

Quantity of serum apparently greater in blood from splenic vein: but what separates in 24 hours no just criterion.

In the course of these experiments, an attempt was made to ascertain whether the blood in the splenic vein has a greater proportion of serum than in the other veins of the body, and the general results were in favour of such an opinion; but it will appear, from what follows, that the quantity of serum separated in twenty-four hours is by no means a juſt criterion of the proportion, which the blood contains.

Exp. 1.

Experiment 1. Three ounces of blood from the arm of a healthy person were received into a graduated glass vessel, previously cooled to the temperature of 32°, three more into a second glass of the temperature of 50°, and three into a third at 70°. The three glasses were brought into a room, the temperature of which varied from 40° to 50°. At the end of nineteen hours, the serum was found in the following quantities.

In the glass at 32°	9 drams.
50°	11
70°	10

The blood did not flow so freely into the glass at the highest temperature, as into the other two.

Exp. 2.

Exp. 2. This experiment was repeated, and the serum examined at the end of forty-three hours.

In the glass at 32°	12 drams.
50°	12
70°	13

Exp. 3.

Exp. 3. It was repeated, and the serum examined at the end of 67 hours.

In the glass at 32°	11 drams.
50°	11½
70°	11½

Exp. 4.

Exp. 4. It was repeated, and the serum measured at the end of ninety hours.

In the glass at 32°	11½ drams.
50°	13
70°	10½

The blood did not flow so readily into the glass at the highest temperature as into the other two.

Most serum separates from blood received into a warm vessel, and flowing freely.

From these experiments it appears, that the serum separates in larger quantity, when the blood is received into a vessel at the temperature of 70 degrees, than at 50° or 32°: this, however, is prevented from taking place by the blood not flowing readily from the vein.

From the experiments on the spleen contained in this and the foregoing paper, the following facts appear to have been ascertained.

Spleen in two different states:

That the spleen is met with in two very different states, one which may be termed the distended, and the other the contracted; and that in the one its size is double what it is in the other. In the distended state there is a distinct appearance of cells containing a limpid fluid, distinguishable by the naked eye; in the contracted, these only become distinct when seen through a magnifying glass. The distended state takes place when the stomach has received unusual quantities of liquids before the animal's death; and the contracted state, when the animal has been kept several days without any drink before the spleen is examined.

dependent on drinking.

That the trunk of the splenic vein (of the hog) is more than five times the size of the trunk of the splenic artery.

Splenic vein 5 times as large as the artery.

That, when the pylorus is secured, coloured liquids pass from the cardiac portion of the stomach into the circulation of the blood, and go off by the urine; and while this is going on, the spleen is in its most distended state, and the colouring matter is found in its juices, although it is not to be detected in those of the liver. The colouring matter cannot therefore be conveyed to the spleen through the common absorbents of the stomach, which lead to the thoracic duct.

Liquids pass from the cardiac portion of the stomach to the spleen.

That, when the pylorus is open, the colouring matter under the circumstances above mentioned is equally detected in the spleen.

That, when the spleen is in this state, the blood in the splenic vein has its serum more strongly impregnated with the colouring matter, than that of the blood in the other veins of the body; and when the stomach is kept without liquids, although colouring matter is carried into the system from the intestinal canal by the ordinary channels, no particular evidence of it is met with in the spleen or its veins.

Colouring matter found in the serum from the splenic vein, if the stomach contain a liquid.

That the cæcum and the portion of the colon immediately beyond it are found (in the ass) to be at all times filled with liquids, even when none has been received into the stomach for several days, and there is a greater number of absorbent vessels for carrying liquids from the colon into the thoracic duct, than from any other part of the body.

Blood vessels occasionally supplied with liquids from the colon.

The colon is therefore a reservoir, from which the blood vessels are occasionally supplied with liquids.

Mr. Sewell informs me, that the same observation applies in a still greater degree to the horse.

Liquids drunk reach the bladder in a few minutes.

That coloured liquids taken into the human stomach, under some circumstances, begin to pass off by urine in seventeen minutes, continue to do so for some hours, and then disappear; they are again met with in the urine, after the colouring matter is known to have arrived at the great intestines, by its passing off by the bowels.

From the above facts, the following conclusions may be drawn.

Liquids conveyed in part through the spleen.

That the liquids received into the stomach, beyond what are employed for digestion, are not wholly carried out of it by the common absorbents of the stomach, or the canal of the intestines, but are partly conveyed through the medium of the spleen into the circulation of the liver.

The communicating vessels not yet discovered.

The vessels which communicate between the stomach and the spleen have not been discovered; but if it is proved, that the colouring matter of the contents of the stomach is met with in greater quantity in the spleen, and in the vein which goes from that organ to the liver, than in the other veins of the body, there appears to be no other mode in which it can arrive there, but by means of such vessels; and the two different states of the spleen, which correspond with the quantities of liquids that pass from the stomach, are strongly in favour of the existence of such a channel.

Hence people in the habit of drinking have the spleen and liver diseased.

This communication between the cardiac portion of the stomach and the spleen will explain the circumstance of those, who are in the habit of drinking spirituous liquors, having the spleen and liver so frequently diseased, and the diseases of both organs being of the same kind.

The spleen not essential to life but its diseases injure digestion.

This organ is not essential to life, its office being of a secondary kind; but when it is materially diseased, or entirely removed, digestion must be disturbed. The extent to which this takes place cannot be accurately known from experiments on quadrupeds, and the instances in which the human spleen has been removed have not been attended to with sufficient accuracy, to afford an explanation of the effects that were produced on the stomach.

V.

Experiments on machines for raising coal required.

improvement of the machines in use for raising coal from the mines has long been a desideratum of the for the Encouragement of Arts, Manufactures, and rce, and they have repeatedly offered a premium for pose.

Those in common use.

e in general use (from the increased expense of horse are worked by a steam engine, attached to a crank ty-one inches radius, wedged on a shaft along with eel, eleven or twelve feet in diameter, and pinion of eleven teeth, which latter works in another of ur teeth, on the shaft of which is a plain cylindrical rom four to six feet diameter, and nine or ten feet Some have barrels formed of frustums of cones, (the ers of which are in the proportion of about five to nited at their bases, and of various diameters. The both kinds are placed at right angles with the cen- of the pit, and at each end a rope of six inches in erence is made fast by a staple, which ropes work rary directions at the same time) over two pulleys, in a frame parallel to each other, and at an equal from the centre of the pit; to the ends of these e baskets of coal and ore to be raised are hooked.

Defect in the form of the barrel.

implicity of their general structure is such as, per- ot to admit of any considerable improvement; but s of the barrels are very defective.

utting one of these machines in motion each rope

Cylindrical barrel.

three sides. Upon the cylindrical barrel the load always tends, from gravitation, towards the nearest point of contact with the centre of motion of the barrel, and, in consequence, the ascending rope at first bends around it in receding coils from the subtending side of the rectangle, diminishing their distances as they approach the nearest point of contact, (where the rope crosses the centres of the pulley and barrel at right angles), thereby leaving a great part of the latter uncovered by the rope, and hence the necessity of such long ones; afterward coiling hard against itself as it approaches the other side of the triangle, to its great injury in wear.

Conical in improper proportions.

The barrels formed of frustums of cones, united at their bases, the perimeters of which are in the proportion of about five to four, are equally defective, on account of the rope, for the reason before mentioned, binding hard against itself, and even sometimes, (in wet weather, when its rigidity is increased by absorption of water,) folding at first in receding coils, and afterward so hard against itself as to force those receding coils to slip suddenly towards the small perimeter of the cone, thereby making a large portion of the rope to descend the pit in an instant, breaking the rope by the sudden jerk, and frequently causing the immediate destruction of the men who may be ascending the pit at the time, or dashing to pieces the basket and its contents.

Danger.

Disadvantages.

Beside the unnecessary expense arising from the use of hempen ropes, and the breakage of chains when applied in the common way, the forms of the barrels are quite erroneous in principle. Some are cylindrical; others formed of frustums of cones united at their bases, without any determinate proportion in their perimeters, or regard to the weight of the rope or chain working thereon, both of which are absolutely necessary to acquire a maximum effect.

Proper proportions.

The convex surface of a frustum of a cone is equal to the convex surface of a cylinder of the same altitude, having its circumference equal to half the sum of the perimeters of the frustum: and circumferences of circles being to one another as their diameters, the surface of a barrel formed of two frustums of right cones (united at their bases), each 64 inches diameter at one end, 32 at the other, and when the former is hooked to the end of the latter, sed from the bottom of the pit (the opposite chain bent on the barrel), $a + b =$ the counterpoise re- at 24 inches radius; and when it is wound up to the he descending part of the opposite chain hanging the pit), $a - b =$ the counterpoise required at the adius.

and with two frustums of cones.

the barrel formed of frustums of right cones, when ad is eased from the bottom of the pit, it and the are suspended from one of the smaller perimeters pposite chain being bent on the barrel), $\frac{a}{2} + \frac{b}{2} =$ unterpoise required at 32 inches radius; and when it nd to the top of the pit, it is suspended from the perimeter of one frustum, while the descending part opposite chain is hanging down the pit from the smaller ter of the other, and in that position $a - \frac{b}{2} =$ the rpoise required at the same radius.

This applied in practice.

sequently, by supposing *a*, the weight of the basket , to be 800lbs. and *b*, the weight of the descending the chain, 400lbs. (these are the weights which we dopted here), we have the counterpoise required upon indrical barrel, at 24 inches radius, 1200lbs. when ket of coals is at the bottom of the pit, and 400lbs. t is at the top; but upon the barrel formed of frus- f right cones, the counterpoise required at 32 inches of right cones, at 24 inches radius. Again, as the descending part of a chain + a basket of coal of double its weight, unbending out of equidistant grooves from the base of a frustum of a right cone towards its smaller perimeter, balances in every revolution of the barrel a chain of equal weight + a basket of coal of double its weight, bending into equidistant grooves from the smaller perimeter of a similar frustum towards its base, the counterpoise required must be equal in all parts of the descent.

Maximum of effect.

So that by making the weight of the basket of coal to that of the chain, and the perimeters of the frustums of cones, which form the barrel, to each other in the proportion of two to one, *a maximum is obtained, by which a barrel of this description requires one third less momentum, (and consequently one third less expense), to work it than a cylindrical one.*

Barrels.

The barrels are made by nailing two or three inch planks upon wooden or iron curves, as in the common way; and afterward folded spirally with wrought iron tire, so as to leave a vacancy of about half an inch between each fold, for the lower part of the ellipses of those links of the chain which work vertically to move in, and keep the coils at an equal distance from each other.

Iron work.

The wrought iron tire is of two kinds, the one for conical, and the other for cylindrical barrels; the cross section of that for the barrel formed of frustums of cones is nearly a parallelogram 1¼ inch by ⅝ths; out of the upper part of which about one fourth of an ellipsis is taken, to form a horizontal bearing for those links of the chain, which lie flat upon the tire; the cross section of the latter is a rectangle, 1¼ inch by 2½ inch. Both are rolled into their proper form, and holes of a quarter of an inch diameter punched therein, at a foot from each other, for the purpose of nailing them to the planking of the barrels.

Chains working in grooves a recent practice.

As the method of working chains in grooves has only been in use about three years and a half, it is impossible to give a certain idea in respect to their durability. In all that time not a single link has broke, or the least accident occurred therefrom, though Messrs. T. W. and B. Botfield, have nearly three thousand feet in daily motion at this manufactory.

The wear has also been so trifling, that I conceive
ill sooner fail from oxidation than attrition: for al-
the machines for raising coal and ore from the mines
ıse twelve hours in the day, the brown oxide of iron
upon the links by exposure to the atmosphere is sel-
sturbed by the motion of the chain.

Cast iron barrels unnecessary.

method of folding wooden barrels with wrought iron
s away the necessity of cast iron ones, and may be
to every wooden barrel now in use at a small expense,
be seen by the estimate which is subjoined.

e are now at work in the mines of this manufactory
achines, with wooden barrels folded with wrought
, one cylindrical, and three formed of frustums of
aising upwards of eight hundred tons of coal and
per week from pits of about eighty yards deep; and
hers are in hand.

Chains used instead of hempen ropes.

k forward with confidence to the general substitution
ns for hempen ropes at all our mines and manufac-
matter of importance to the British empire, as it
siderably lessen the consumption of hemp, and ren-
ore abundant for the exigencies of the Navy.

ing to give this method of working chains all the
y in my power, I will obviate all apparent (for there
eal) difficulties, which may occur to any person in
plication, on his stating them in a letter *post paid*
d to me here.

I am, Sir,

Your most obedient Servant,

GILBERT GILPIN.

Comparative estimate of expense.

of tarred ropes for a machine for raising coal and
m a pit eighty yards deep, for three years and four
s.

Ropes.

	£	s.	d.
es each 110 yards long, 6 inches in circumference, and 5lbs. per yard, 5500lbs. at er lb	183	6	8
10 worn out ropes 2750lbs. at 1*d*. lb..	11	9	2
ense of ropes for 3 years and 4 months	£171	17	6

Expense of chains for a machine for raising coal and or from a pit eighty yards deep.

Chains.

Two chains each 110 yards long, formed of ⅜ inch iron, 28 links to the yard, and weighing 5lbs. per yard, 1100lbs. at 6*d*. per lb········	27	10
180 yards of wrought iron tire, with the holes punched therein weighing 7lbs. per yard, at 1*s*. 6*d*. per yard ·······················	13	10
540 nails for the tire, 27lbs. at 6*d*. per lb······	0	13
Workmanship, nailing the tire on the barrel, 180 yards at 2½*d*. per yard····················	1	17
	£43	11

The above chains and tire have been at work three year and four months, and do not appear to be one fourth worn

Ropes retained in part on account of the men's prejudices.

Messrs. T. W. and B. Botfield annex a certificate, that they have now at work at their mines four of Mr. Gilpin's machines, one with a cylindrical barrel, and three formed of frustums of cones; which machines they conceive to be superior to any hitherto known or in use, and producing their effect at a much less expence. To this Mr. Gilpin subjoins

You will please to observe, that of the four machines now in use, two only work with *two chains each*, and they are both formed of frustums of cones; the other two, the one with a cylindrical barrel, and the other a frustum of a cone, have each a chain at one end, and a patent flat rope at the other. We are induced to adopt the latter plan, to do away by *degrees* the prejudices, which miners and colliers have imbibed against chains, from accidents which they have been witnesses to in the common way of working. Though the causes of similar accidents are entirely done away by the new method of working, some little of the old prejudice remains; a thing not to be wondered at, when we consider the uninformed state of this description of men, arising from a life spent in the dark recesses of mines; and, as it were, cut off from the rest of society.

From

ce to the Engraving of Mr. Gilbert Gilpin's im-
l Machine for raising Coal, Ore, &c. Pl. 3, fig. 1,
4.

Explanation of the plate.

. *a*. A crank to which the connecting rod is fixed
the machine to the steam-engine which works it.
wheel of 13 teeth, wedged about the same shaft with
k, and which works into the wheel *d*.
fly wheel 11 feet in diameter, wedged upon the same
the wheel *b*.
wheel of 64 teeth, wedged upon the same shaft as
el, into which the wheel *b* works.
wooden barrel, formed of two frustums of cones
base to base, and folded spirally with wrought iron
ch keeps the links of the chains at right angles with
er, and with the grooves in the pulleys.
e reeling-post and its lever, for disengaging the bar-
the steam-engine, when the men are to be let
to the pit by means of the break.
break wheel, break and lever, for regulating the
of the barrel when disengaged from the steam en-
d in the act of lowering the miners into the pit.
he frame on which the machine is erected.
. 2. The pit-frame, for supporting the pulleys.
pit represented by a circle, part of which is shown
d part by dotted lines.

the coal and ore are landed from the chain at the pit head moving on four small iron wheels.

nn. Baskets on which the coal and ore are raised from the pits.

o. The hook which goes into the staple of the basket to draw it forward when lowering on to the tacking.

After the basket is lowered, the tacking is drawn forward by two girls to the edge of the frame, which is laid level with the ground on its outside. and near to which the coal and ore are loaded into waggons, and afterward drawn upon iron rail-ways to the furnaces, forges, &c.

Fig. 3. A section of a part of the barrel and tire, showing the manner the links of the chain lie on it, on a scale of 3 inches to the foot.

Fig. 4. A section of the pulley, with a link of the chain lying in it.

In a large machine the barrel is fixed 24 or 25 yards from the pit.

VI.

Remarks on Mr. Gough's *Essay on Polygonal Numbers: by* P. Barlow, *Esq.*

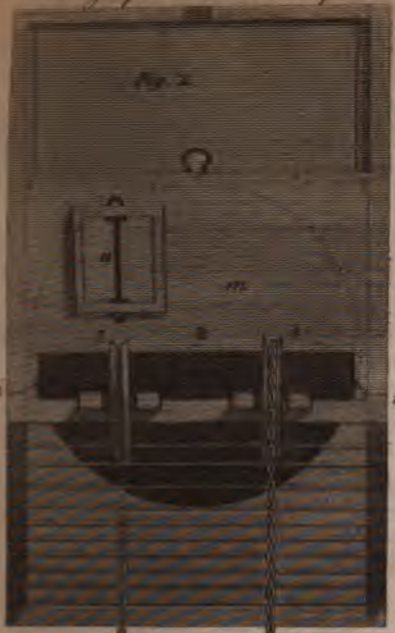
To the Editor of the Philosophical Journal.

SIR,

On Fermat's proposition on polygonal numbers.

IN your number for July, the first article is an essay by John Gough, Esq., in which he has attempted the demonst.ation of a very curious and general property of numbers; but as it appears to want that perspicuity and simplicity, which are the distinguishing beauty of mathematical reasoning, I have drawn together the following observations upon it; which, if you think proper to insert them, will give your readers an opportunity of judging of its merits, more particularly than they may have hitherto done. It will also

Mr. Gilpin's Machine for raising Coals, Ore, &c.



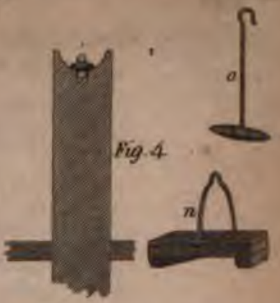


Fig. 4

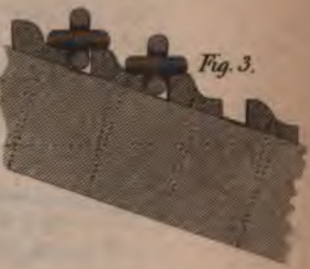


Fig. 3.

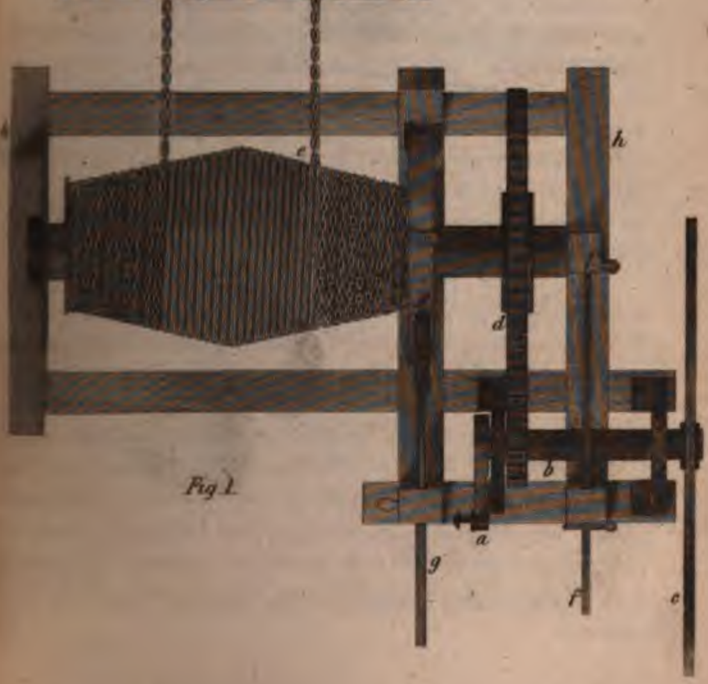


Fig. 1.

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TILDEN FOUNDATIONS
L.

a so afford Mr. G. an opportunity of explaining the ambiguous parts; and will at the same time much oblige

On Fermat's proposition on polygonal numbers,

Yours, &c.

P. BARLOW.

Royal Mil. Academy,
Sept. 13, 1808.

The curious theorem, which Mr. Gough has undertaken to demonstrate, was first announced by Fermat, in one of his notes at page 180 of his edition of Diophantus; and the demonstration for the particular case of squares was given first by Lagrange, in the *(Mém. de Berlin,* 1770*)*, and afterwards in a simpler form by Euler, in the *(Acta Petrop. Ann.* 1777*)*, as we are informed by le Gendre, in his *Essai sur la Théorie des Nombres*, at page 202; where there is also given a demonstration for the same particular case. Le Gendre has likewise in another part extended it to triangular numbers, and this is the most that has ever been done by any mathematician. If therefore the ingenious author of the abovementioned essay has failed in his demonstration, he has the satisfaction of having failed in an attempt, in which many of the ablest mathematicians in Europe have succeeded no better than himself; and if, on the contrary, he can clear up those parts, which appear at present to be defective, the greater degree of merit will be due to his ingenuity and ability, of which I have always entertained the highest opinion: and I feel confident, that he will not mistake my intentions in the following criticism, but rather attribute it to my love for mathematical truth, than to any invidious desire of criticising his paper.

The first three propositions and their corollaries are in themselves correct, although I am at a loss to see in what manner they are intended to be applied to the general demonstration. The first part that I shall examine is the conclusion drawn at cor. 2, prop. 4. In cor. 1 of the same prop. it is proved, that e, which is taken to represent any aggregate of polygonals of the denomination m, is of the form $e = p + \overline{m - 2}.\,s$; and then in cor. 2, having shown that every natural number is also of the same form, $p + \overline{m - 2}.\,s$, the converse of the prop. is inferred to be true likewise;

On Fermat's proposition on polygonal numbers.

likewise; namely, that every number is the aggregate of polygonals. Now it is easily seen, that this is false reasoning. Our author might, with as much propriety, have said, that every natural number is either even or odd, and every aggregate of polygonals being also either even or odd, therefore every natural number is the aggregate of polygonals: or, to put it in a stronger light, every natural number is of the form $p + \overline{m - 2}.\, s$; and, every square number being also of the form $p + \overline{m - 2}.\, s$, therefore every natural number is a square number. No person can for a moment fail of detecting in those two last cases the fallacy of this reasoning, nor of perceiving the strict analogy it bears with that made use of in the cor. abovementioned. It is to be observed, that I do not object to the conclusion, but to the manner of obtaining it; for all that is drawn from the first four propositions and their corollaries might have been granted at first as a postulate, if any use could have been made of it in the general demonstration.

For unity is a polygon of every denomination, and every natural number is composed of a number of units, therefore every natural number is composed of a number of polygons of any denomination *m*, consequently every natural number is either a polygon of a given denomination *m*, or may be resolved into polygons of that denomination; the number of those polygons being unlimited, as in the corollary alluded to.

The next place, where any conclusion is drawn, is in the cor. to prop. 6, where it is said, that *If* $e = y + t$, can be resolved into polygons, the number of which $= m - f$, $e + f$ may be resolved into m polygons of the same denomination. Now either this supposition is necessary to complete the demonstration, or it is not: if it is not necessary, it ought to have been omitted; if it is necessary, it ought to have been shown (but it no where is in the demonstration) that $e = y + t$ may be resolved into $m - f$ polygons, because the conclusion depends upon this supposition, and if the supposition is *true*, the conclusion is *true*; on the contrary, if the supposition is *false*, the conclusion must necessarily *be so likewise*. This language is at all events too vague for mathematical reasoning. I am willing to allow that, *if e can* be resolved into $m-f$ polygons, $e+f$ may be resolved into m polygons; but *if e cannot* be resolved into $m-f$ polygons, what proof have we, that $e+f$ can be resolved into m polygons? And that there are many such cases is evident: thus, 14 cannot be resolved into less than *three* or m triangular numbers, nor 23 into less than *four* or m squares.

On Fermat's proposition on polygonal numbers.

This ought to be explained, the importance of the proposition demands it, the last labours of *Euler, Lagrange,* and *le Gendre* demand it also, and a few more pages may very well be afforded to complete the demonstration.

The remaining part of the essay goes on to show, how any given number may be resolved into polygonal numbers of any given denomination; but, from the examples there given, it does not appear to possess any advantage over the usual method of trials; and even if it did, it is of no use in demonstrating the proposition, for showing how a thing is to be done is very different from showing it may always be done.

Upon the whole therefore we may conclude, that for the *present,* the celebrated theorem of Fermat is without a demonstration, and that its importance, as containing one of the most curious properties of numbers, renders it worthy the attention of mathematicians.

VII.

Some farther Remarks on the Doctrines of Chance, in a Letter from a Correspondent.

SIR,

HAVING observed a letter in the last number of your valuable publication, from a correspondent who has assumed the signature of Opsimath, in which some doubts are expressed respecting the elementary Doctrines of Chance, and a request to yourself, or any of your correspondents, either to confute or to confirm his objections, I have ventured to offer the following remarks; though certainly with some diffidence, being

Certain doctrines of chance questioned by a former correspondent.

being apprehensive from your having declined favouring us with your remarks, that your view of the subject might not be very dissimilar to your correspondent's.

The case objected to.

Opsimath admits de Moivre's first case, viz. that if any one were to undertake to throw an ace in one throw with one die, he would have $\frac{1}{6}$ of all the possible chances in his favour, and the remaining $\frac{5}{6}$ against him: but objects to the second case, viz. that, if he were to undertake to do it in two throws with one die (or, which is certainly the same thing, in one throw with 2 dice) that the chances in his favour are $\frac{11}{36}$ and $\frac{25}{36}$ against it; alleging as a reason, that two equal chances are twice as good as one, and that of course it should be $\frac{12}{36}$ instead of $\frac{11}{36}$.

Attempt to defend it.

This reasoning is correct if the chances are of equal value: but this I apprehend is not the case, the second chance being less than the first by the probability of the first's succeeding; and as a confirmation of de Moivre's doctrine being correct, it appears by the following statement, that of all the 36 possible combinations with 2 dice, there are but 11 throws which give an ace or any other particular number. Let 1 die be called A, the other B; then may be thrown with

A	B	A	B	A	B	A	B	A	B	A	B
1	1	2	1	3	1	4	1	5	1	6	1
1	2	2	2	3	2	4	2	5	2	6	2
1	3	2	3	3	3	4	3	5	3	6	3
1	4	2	4	3	4	4	4	5	4	6	4
1	5	2	5	3	5	4	5	5	5	6	5
1	6	2	6	3	6	4	6	5	6	6	6

and again, as the chance of throwing an ace with one die is admitted by your correspondent to be $\frac{1}{6}$, and of not doing it $\frac{5}{6}$, the chance of not doing it with either of two dice is $\frac{5}{6} \times \frac{5}{6} = \frac{25}{36}$, and this subtracted from unity, which represents the certainty of an ace being either thrown or not thrown, gives $\frac{11}{36}$ as above.

The argument pursued.

Opsimath farther objects to this statement, and says, if we proceed according to the above method, the probability of throwing an ace with one die in 6 throws does not amount to $\frac{6}{6}$, or a certainty. Nor should it: for were this the case, he might undertake to pay any sum, provided he did not do it in 6 throws with 1 die, or in 1 throw with 6 dice, which I think he would be very unwilling to do. The fact is, that out of the 46656 possible combinations with 6 dice, there are only 31031 throws that produce an ace, or any other particular number; which, if he will take the trouble, he may convince himself of, by trying all the combinations, as in the preceding statement of the 2 dice, or according to the method before given, viz.

Probability of not throwing an ace

with 1 die .... $\frac{5}{6}$ of doing it $\frac{1}{6}$
with 2 dice $\frac{5}{6}\times\frac{5}{6}=$ $\frac{25}{36}$ .... $\frac{11}{36}$
with 3 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$ $\frac{125}{216}$ .... $\frac{91}{216}$
with 4 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$ $\frac{625}{1296}$ .... $\frac{671}{1296}$
with 5 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=$ $\frac{3125}{7776}$ .... $\frac{4651}{7776}$
with 6 dice $\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}\times\frac{5}{6}=\frac{15625}{46656}$ .... $\frac{31031}{46656}$

Similar reasoning applied to two halfpence.

With respect to throwing a head with a halfpenny in 2 throws (or with 2 halfpence in 1 throw, being the same thing) it ought, according to his view of the subject, if I understand him right, to amount to a certainty; as there are but two ways in which a halfpenny can be thrown, and there being two halfpence to do it with. He appears however to be satisfied with de Moivre's value of the chance, viz. $\frac{3}{4}$, which is the true one, for in the 4 ways in which 2 halfpence may be thrown, there are only 3 which give a head; for with the first

may be thrown a head, and with the second a head,
head, ... tail,
tail, ... head,
tail, ... tail.

I am, Sir,
Your constant reader,
and most obedient servant,
B. H.

10, *Millman Street, Bedford Row,*
14 *Sept.* 1808.

REMARK.

REMARK.

The mistake has arisen from confounding several throws of one die with one throw of several dice.

B. H. appears to be led into an errour by supposing, that the chances for throwing an ace in six throws of one die and six throws of another, are the same thing with the chance of throwing an ace in six throws of two dice; but this is not the fact. Six throws of one die and six throws of another are clearly equal to twelve throws of one die, and this chance I apprehend will not be denied to be equal to two. In throwing two dice thirty-six times, it appears by the table of B. H. himself, which is perfectly accurate, that the thrower may calculate upon throwing twelve aces, as he might by throwing one die seventy-two times: but here is the difference; in throwing one die seventy-two times, he has a right to reckon on an ace being turned up in twelve of the throws; in throwing the two dice thirty-six times however, he can reckon on no more than eleven throws in which an ace will be turned up, because in one of the throws two aces will come together, and consequently one will be lost, which evidently cannot be the case when the two dice are thrown in succession.

With regard to the throws of a halfpenny the reasoning is precisely the same; nor does Opsimath appear more inclined to acquiesce in the assertion, that the chance of throwing a head with one halfpenny in two throws is only ¾: though he would probably allow this to be the true value of the chance of throwing a head at one throw with two halfpence. C.

VIII.

Description of a secure Sailing Boat, or Life Boat. By Mr. CHRISTOPHER WILSON, *Richard Street, Commercial Road*[*].

SIR,

Neutral built self-balanced boat.

HEREWITH you will receive drawings of a neutral built self-balanced boat, with an explanation, which I request

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[*] Trans. of Soc. of Arts, vol. XXV, p. 55. The gold medal was voted to Mr. Wilson for this invention.

quest you will have the goodness to lay before the Society for the Encouragement of Arts, &c., for their inspection and approbation. I have made the explanation as clear as I can. Its construction will obviate the danger of its being overset by persons crowding on one side, in getting in or out of the boat; it will facilitate the landing of men on shore or in boarding ships, and will carry a much greater press of sail without danger.

Its advantages.

As to the building part, I think that may be easily understood. My boat was made by men that had never before seen a boat built, and I flatter myself the Society will approve of it.

I am, Sir,

Your most obedient humble servant,

CHRISTOPHER WILSON.

An Explanation of the Engravings of the neutral-built self-balanced Boat.

Method of its construction.

By the term neutral is meant, what is neither of the two present modes now in use, i. e. clincher and carvel, but both united, viz. clincher in the inside and carvel on the outside, which neutralizes both the two into a third; and as every thing has a distinguishing name, I have taken the liberty to present it to the public, under the name of a neutral boat.

The two modes of clincher and carvel-built have each their separate advantages and disadvantages in regard to each other.

Advantages of clincher building.

I shall begin with the clincher first. As the sides of the planks are firmly fastened to each other, by lapping over and rivetting, they are much stronger than if the edges only butted; and they have the property of being made tight without caulking, only in the huddings and keel seams, and are much lighter than carvel-built boats, and more adapted for many uses; besides saving the difference between thick and thin plank. But they have their disadvantages also; in the first place, both unfair sides and unfair water lines, which makes them liable to be injured by other bodies they

Its disadvantages.

come in contact with, and have the edges of the planks broke so as to make a leak*, which would not happen to a smooth-sided boat, neither can the uneven side move so well through the water, on account of its various resistances†. They have also this disadvantage, that if damaged, they require the skill of a professional workman to repair them.

Advantages of carvel building.

The carvel built boats have the advantage of having smooth sides and fair water lines, together with having the planks of an equal thickness all over the boat, which makes them less liable to receive injuries when meeting with other bodies, and more adapted to move in the water, by their fair sides and fair water lines. They are also more readily repaired: if a professional boat-builder is not at hand, it can be done by a common shipwright, or any workman that is used to wood work.

Its disadvantages.

But they have also their disadvantages; in the first instance they are under the necessity of being built of plank of a great thickness to stand caulking; at the same time they require larger timbers, which makes them heavy and unfit for many uses, and also a great consumption of timber on account of the thickness of the plank necessary. They are also more subject to leaks from various causes than clincher-built boats.

Neutral building.

We will now look to the neutral system, and see if both their advantages are not united, and both the disadvantages got clear of.

Pl. IV, fig. 2, shows the section of the fore part of a boat. The longitudinal slips are represented lighter coloured, and placed over the joints where the edges of the planks meet; they must be rivetted on to each adjoining plank, near the edge, in the same manner as clincher-built vessels, with a sufficient quantity of blair, made of tar and flocks, such as is in common use in the north of England, (or any ılking), between the slips and planks, which will eep them tight, as long as the boat remains un- ɔr the planks not worn through. These slips, each ·etted to the two adjoining edges of the planks, as ı Fig. 4, will make the joint as strong as the joint ımon clincher-built boat, and as tight, without the ny external damage. These joints have also this ;e, that the planks will not have their sides bevelled ɔe of an equal thickness from edge to edge, which is case in clincher-built vessels, for at the ends they bevelled away, so as not to bear clenching. By ral system two inches in the breadth of each plank aved in the laps, which may be considerable in the ɔn of plank. I set little value on the slips, as there ; a sufficiency of waste in cutting the planks to a orm.

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* In the next paragraph but one carvel built boats are said to be more subject to leaks. C.

† This does not appear to be the fact. Clincher built vessels are so superior to others in sailing, that, by an act of parliament passed many years ago for the prevention of smuggling, they were declared illegal beyond certain dimensions. C.

t of this construction has all the strength of one ·built, and can be made as light or lighter. It is ı the disadvantages of irregular outsides, and from :ulty of repairing, which in this can be performed ·ommon workman in wood, as I have found by ex- . A boat built this way has a fair and smooth out- ıas all the advantages of a carvel-built one, at the ıe it is clear of the disadvantages of being loaded ıecessary wood, which makes the carvel-work very ıe liability of leaks, and frequent want of caulking.

Common defect above the keel remedied.

; one evil, which both carvel and clincher built ve in common, that of having keel seams, and a between the sand or garboard streak, and the upper :he keel, which soon gets filled with dirt, and re-), which naturally retains moisture, and speedily wood. In this mode that evil is removed, by hav- ınidship plank bolted on to the keel, wide enough over each side of the keel to clinch the slips on, only removes the evil, but saves a great deal of n making the rabbets in the keel, and various be- ın the sand streaks, which must be done by a good).

boats require no larger timbers than common built boats, as the timbers need no greater notches, but with this difference, that these timbers will catch the slips that are rivetted over the joints of the planks each way, and so the timbers and slips will brace one another, and add an additional strength; but in the clincher built boats, the timbers catch the laps of the seams only one way, and consequently form no brace whatever.

Applicable to boats, barges, &c.

All I need to explain farther on the neutral system is its application. It can be applied to all open boats, of whatever form or use, to all coal and other barges, lighters, or any vessels used in rivers or canals, and also to all large cutters and luggers, which are now clincher built.

Explanation of Pl. IV, fig. 1, 2, 3, 4.

Explanation of the plate.

Fig. 1, is a bird's eye view of the boat, showing the projecting balance bodies, or hollow sides *a b*, one of which, *a*, is left open to show the partitions, which are placed opposite to each timber, and are water tight; by this means, if one or more should be broken, the rest would keep the vessel buoyant. These partitions gradually lessen towards each end, where the planks unite, so as to make a similar appearance to any other boat when in the water.

Fig. 2, shows the depth and form of the cells or hollows, as they appear in a section of the boat; also the manner in which the slips are placed over the joinings, or seams of the planks.

Fig. 3, is a perspective view of the boat, in which *a b* show the projecting balance bodies, or hollow sides, which would render the boat buoyant if her bottom was staved in. *c*, the lower part or body of the boat, from which the projections commence; *d*, the keel.

Fig. 4, shows the manner in which the planks or timbers of the boat are united; *e f*, are two planks of the boat; *g*, the slip of wood placed over them, and secured to them by the rivets *h h*.

The section (Fig. 2), will best explain the nature and utility of the self-balanced boat. The balance bodies form two separate holds, to put any thing in, such as provision, arms, &c., which are wanted to be kept dry, having locker lids, to open at the top of the different partitions in the holds, as fancy or utility may require; or part of them may be filled with cork shavings, and by that means, if the boat should happen to fill by any accident, she cannot sink.

Boats altered for Government.

In the boat I have altered for Government, the balance bodies (if the interior of the boat was filled with water) would exclude as much water, between the inside of the boat and the outside, as is equal to a body of water of 1 tun, 17 cwt, 2 qrs, which is a great deal more than the weight of men that will go in her, consequently they can run no risk whatever of being drowned; and even if she had a hole through her bottom, she would always keep a sufficient height out of the water either for rowing or sailing.

But the main object is to make her sail and row much faster than other boats, and both on calculation and trial my boat will be found to sail much faster, and with much less danger than other boats.

Advantage of rowing with a longer lever.

I now come to the advantage of rowing.—As the balance sides project a foot beyond the resisting part in the water, there is that leverage on the boat (over a common one), and also the same in the length of the loom of the oar, that is in the inside from the gunwale of the boat, which allows the whole of the oar to be lengthened, and by that means it describes a larger circle in the water, and makes a longer pull: the oars for the Government boat I have made are lengthened from 14 to 18 feet.

This may be effected by projecting spars.

The experiment of having two spars fixed at a distance from a boat's gunwale, and the oars to work from them, has often been tried and found to answer, but this has a great advantage over that method.

Will not roll, and will pitch but little.

There is another advantage or property which this boat has, she cannot roll at sea, but always keeps a level position as far as the surface of the sea will allow; she may heel but not roll, as the balances are always ready to catch either way, and the opposite one assists the other by its weight out of water and gravitation; neither can this boat pitch like another, for the balance bodies being out of the water, and the breadth of six feet only in the water, it can only act with a gravity on the water, equal to a boat of the weight of six feet but as the resistance of the water upwards equal to a boat of eight feet wide.

Or I may make this mechanical simile: Suppose a workman uses a chissel to smooth a surface of wood, by laying too great a stress on the tool it will go too far into the wood for him to force it along in the direction wanted, but put that chissel into a stock like a plane-stock, and set it to the depth required, then the stock will prevent its going too far in, and he can work easily though the plane be pressed on ever so hard. A view of the engraving will elucidate this comparison, as the balance bodies lie parallel with the surface of the water lengthways. The national importance of such boats I leave to the public to decide. I must here observe, that my plan contains two distinct and separate improvements, viz. my neutral mode of building, and the application of the balance bodies.

Two separate improvements of different application.

The first improvement relates to the *building* of boats, barges, &c., in general. The second is only partial, and applicable to boats of peculiar descriptions or uses; that is, all such as are wanted for dispatch, safety, or pleasure, or occasionally for life boats: as there can be no question of the self-balanced boats, built upon my plan, rowing and sailing faster than other boats, and they may be used to go to sea when others cannot; but the application of the balance bodies is not meant as a general one, as it is not fit for vessels of burden that are sometimes light, and at others heavy laden, when the difference of the draught of water is considerable.

CHRISTOPHER WILSON.

Opinions of the advantages of this mode.

CERTIFICATE.—We whose names are hereunto subscribed have examined the boat building on Mr. Wilson's plan, (which he calls the neutral plan) and are of opinion, that it will be attended with many advantages.

The boats can be built as light as those that are clincher built, preserving a smooth surface, and will not require caulking; and they can be easily repaired by any carpenter.

The advantage this boat possesses by having air gunwales are obvious, and from the partial trial we have had of the boat's sailing which he has altered, we are of opinion, that his improvement in the keel and formation of the boat's bottom will give her greater stability than other boats of the same dimensions, with the properties of sailing well and drawing very little water.

MALCOLM COWAN, R. N.
JAMES NICOLSON, R. N.

GENTLEMEN,

PERMIT me to present my thanks and acknowledgments for the truly polite and distinguished manner in which (though a stranger) you have permitted me to visit your Committee; the Society of which the same is formed I hold in the highest estimation, and have deeply to regret the distance, that prevents my offering myself a candidate for a seat among you.

The last time I had the honour of attending your Committee, Mr. Wilson's new life boat became the subject of discussion, the operation of which you did me the honour of requesting me to acquaint you of as soon as an opportunity presented itself for a fair trial of her at sea.

Trial of the boat in a gale of wind.

About three o'clock in the afternoon of Friday last, the tide being about quarter flood, and the wind at south-west, blowing excessively hard, an object was discovered in the offing at about two leagues distance, bearing from the piers of Newhaven W. S. W., which had the appearance of a vessel waterlogged, and with only her foremast standing. This induced Mr. Thomas Tasker (the person whom I appointed master of the boat, and which I have named the *Adeline)* with seven others, to put to sea, with a view of rendering assistance to the supposed distressed vessel, and although the breakers were tremendous, and the sea without them running very high, the boat under the management of the crew beforementioned, ranged as coxswain, six sitters, and a bowman, went out of the harbour in a very lively style, and soon came up with the object in pursuit, which proved to be a beacon, or lighthouse, of a singular construction, triangularly built, and clench-board covered in its floating case, with a mast rigged out in the centre of

one

one of the sides, and supposed to have broken adrift from the enemy's coast by the boisterous weather: finding its magnitude too vast for their strength to tow, and the evening approaching, they returned. Numbers of persons were assembled on the piers to witness the action, power, and performance of the boat, who were highly pleased and gratified. I was not present myself, but the next morning one of the crew was sent to me from Newhaven to this place, who stated, that the whole of them were so fully satisfied with the safety and superior powers of the boat, that they shall not be afraid to put to sea in any weather, when the distresses of their fellow creatures claim their exertions and assistance. They particularly observed, she, with the six oars manned, pulled extremely light and easy through the water, and that though the breakers they pulled through, and the heavy seas they rode over were awful, she did not ship ten gallons of water the whole trip, neither were the men wet on the seats. We have now at Newhaven one of Mr. Greathead's boats, provided by subscription, but from the difficulty of getting her to sea, and her weight and construction rendering it almost impossible to pull her through the broken water, it is very improbable she will ever be used.

Mr. Greathead's boats much inferior.

My opinion is, that Mr. Wilson's boat will answer. Its cost I conceive will exceed £150, including the building and fitting her out.

I have the honour to subscribe myself with the greatest respect,

Gentlemen,
Your obliged and most
obedient humble servant,
WILLIAM BALCOMBE LANGRIDGE.

P. S. I should have observed, that the crew pulled her *stern on* at every sea, and that such water, as in general fills over the bow of ordinary boats, is received by the fore part of her flammings, or floor of extended sides, and sent or dispersed sideways.

IX.

Description of a Capstan, that works without requiring the Messenger or Cable coiled round it to be ever ſurged. By J. WITLEY BOSWELL, *Esq., of Clifford's Inn**.

SIR,

Capstan that does not require the messenger or cable to be surged.

I Request you will lay before the Society of Arts, &c. the model of a capstan contrived by me, which works without requiring the messenger or cable coiled round it to be ever surged, an operation necessary with common capstans, which is always attended with delay, and frequently with danger.

Capstans of this kind can be made by a common shipwright, and would not be liable to be put out of order. They also would not occasion any additional friction or wear to the messenger or cable, in which particulars they would be superior to the other contrivance hitherto brought forward for the same purpose; they also would much facilitate the holding on.

Reasons for not applying to the Navy Board.

The great loss of time and great trouble, which always attend applications to the Navy Board, prevent my attempting to bring the matter before the public through that channel, though I have had the most unequivocal approbation of the capstan from the two gentlemen of that board best qualified to judge of it. I mention this, least it might be thought, that my not applying there first was from any doubt of the goodness of the invention. If the Society should approve of the capstan, I will draw up a more minute account of it for publication.

I am, Sir,
Your very humble servant,
J. W. BOSWELL.

SIR,

Opinions respecting its merit.

I Have examined your model of a capstan, which is calculated to prevent the surging of the messenger when heaving in the cable, it certainly possesses great merit, and the idea to me is quite new.

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* Trans. of Soc. of Arts, vol. XXV, p. 65. For this invention the gold medal was voted to Mr. Boswell.

I am, Sir,
Your humble servant,
WILLIAM RULE.

Somerset-place, November 19, 1806.
To Mr. Boswell.

SIR,

According to your desire, I transcribe the part of the letter from Mr. Peake (Surveyor of the Navy) to me, which relates to the capstan laid before the Society.

Extract of a Letter from Henry Peake, Esq.

" With regard to your ideas on the capstan; I have tried " all I can to find some objection to it, but confess I " hitherto have been foiled, and shall more readily forward " it, if it was only to supersede a plan now creeping into " the service, more expensive, and much worse than one " lately exploded."

As you and the members of the Committee have seen the letter, I imagine further attestation needless relative to it.

No friction between the turns of the cable or messenger.

I request you will mention, that all friction of the revolutions of the cable (or messenger) in passing each other between the barrels of the capstan, must be effectually prevented by the whole thickness of one of the rings that passes betwixt each crossing. I add this because one of the gentlemen of the Committee wished to be informed on this point.

I am, Sir,
Your very respectful humble servant,
J. W. BOSWELL.

SIR,

In obedience to your intimation, that a written explanation of the advantages to be obtained by the use of capstans made according to the model, which I laid before the Society for the Encouragement of Arts, &c., would be acceptable, I send the following, which I hope will make the subject sufficiently clear.

Method of hauling large cables on board a ship.

As few but mariners understand the manner in which cables are hauled aboard in large ships, it will probably render the object of my capstan more manifest, to give some account of this operation.—Cables above a certain diameter are too inflexible, to admit of being coiled round a capstan; in ships where cables of so large dimensions are necessary, a smaller cable is employed for this purpose, which is called the *messenger*, the two ends of which are made fast together so as to form an endless rope, which, as the capstan is turned about, revolves round it in unceasing succession, passing on its course to the head of the ship, and again returning to the capstan. To this returning part of the messenger, the great cable is made fast by a number of small ropes, called nippers, placed at regular intervals; these nippers are applied, as the cable enters the hawse hole, and are again removed as it approaches the capstan, after which it is lowered into the cable tier.

Necessity of surging.

The messenger, or any other rope coiled round the capstan, must descend a space at every revolution, equal to the diameter of the rope or cable used; this circumstance brings the coils in a few turns to the bottom of the capstan, when it can no longer be turned round, till the coils are loosened and raised up to its other extremity, after which the motion proceeds as before. This operation of shifting the place of the coils of the messenger on the capstan is called *surging the messenger:* It always causes considerable delay; and when the messenger chances to slip in changing its position, which sometimes happens, no small danger is incurred by those who are employed about the capstan.

Causes delay and danger.

First attempt to obviate this.

The first method that I know of, used to prevent the necessity of surging, was by placing a horizontal roller beneath the messenger, where it first entered on the capstan, so supported by a frame, in which it turned on gudgeons, that the messenger in passing over it was compelled to force upwards all the coils above the capstan, as it formed a new coil.

This

Disadvantages. This violent forcing of the coils upwards along the barrel of the capstan not only adds considerably to the labour in turning the capstan, but from the great friction which the messenger must suffer in the operation, while pressed so hard against the capstan, (as it must be by the weight of the anchor and strain of the men,) could not but cause a very great wear and injury to the messenger, or other cable wound round the capstan; and that this wear must occasion an expense of no small amount, must be manifest on considering the large sums which the smallest cables used for this purpose cost.

Second attempt. The next method applied to prevent surging was that for which Mr. Plucknet obtained a patent, the specification of which may be seen in the Repertory of Arts, No. 46. In this way a number of upright puppets or lifters, placed round the capstan, were made to rise in succession, as the capstan turned round, by a circular inclined plane placed beneath them, over which their lower extremities moved on friction wheels; and these puppets, as they rose, forced upwards the coils of the messenger on the barrel of the capstan.

This something better. This was a superior method to the first, as the operation of forcing upward the coils was performed more gradually by it; but still the wear of the messenger from the lateral friction in rising against the whelps of the capstan remained undiminished.

Third attempt. The third method used for the same purpose was that proposed by captain Hamilton. It consisted in giving the capstan a conical shape, with an angle so obtuse, that the strain of the messenger forced the coils to ascend along the sloped sides of the barrel. The roller first mentioned was sometimes used with this capstan, of which a full account is inserted in the Repertory of Arts, vol. 2.

Friction as great, and attended with a new inconvenience. The lateral friction, and wear of the messenger against the whelps of the capstan, are equally great in this method as in the others; and it, besides, has the inconvenience of causing the coils to become loose as they ascend; for as the upper part of the barrel is near a third less in diameter than the lower part, the round of the messenger, that tightly embraced the lower part, must exceed the circumference of the upper extremity in the same proportion.

Advantages of In the method of preventing the necessity of surging, which the model I have had the honour of laying before the Society represents, none of the lateral friction of the messenger or cable against the whelps of the capstan, (which all the other methods of effecting the same purpose before mentioned labour under,) can possibly take place, and of course the wear of the messenger occasioned thereby will be entirely avoided in it, while it performs its purpose more smoothly, equally, and with a less moving power than any of them.

the method now proposed.

This described.

My method of preventing the necessity of surging consists in the simple addition of a second smaller barrel or capstan of less dimensions to the large one; beside which it is to be placed in a similar manner, and which need not in general exceed the size of a half-barrel cask. The coils of the messenger are to be passed alternately round the large capstan and this small barrel, but with their direction reversed on the different barrels, so that they may cross each other in the interval between the barrels, in order that they may have the more extensive contact with, and better gripe on each barrel. To keep the coils distinct, and prevent their touching each other in passing from one barrel to the other, projecting rings are fastened round each barrel, at a distance from each other equal to about two diameters of the messenger and the thickness of the ring. These rings should be so fixed on the two barrels, that those on one barrel should be exactly opposite the middle of the intervals between those on the other barrel: and this is the only circumstance, which requires any particular attention in the construction of this capstan. The rings should project about as much as the cable or messenger from the barrels, which may be formed with whelps, and in every other respect, not before mentioned, in the usual manner for capstan barrels, only that I would recommend the whelps to be formed without any inclination inwards at the top, but to stand upright all round, so as to form the body of the capstan in the shape of a polygonal prism, if the intervals between the whelps are filled up, in order that the coils may have equal tension at the top and at the bottom of the barrels, and that the defect which conical barrels cause in this respect may be avoided.

The small barrel should be furnished with falling palls as well as the large one; a fixed iron spindle ascending from the deck will be the best for it, as it will take up less room. This spindle may be secured below the deck, so as to bear any strain, as the small barrel need not be much above half the height of the large barrel; the capstan bars can easily pass over it in heaving round, when it is thought fit to use capstan bars on the same deck with the small barrel. As two turns of the messenger round both barrels will be at least equivalent to three turns round the common capstan, it will hardly ever be necessary to use more than four turns round the two barrels.

How lateral friction is prevented.

The circumstance which prevents the lateral friction of the messenger in my double capstan is, that in it each coil is kept distinct from the rest, and must pass on to the second barrel, before it can gain the next elevation on the first, by which no one coil can have any influence in raising or depressing another; and what each separate coil descends in a single revolution, it regains as much as is necessary in its passage between the barrels, where in the air, and free from all contact with any part of the apparatus, it attains higher elevation without a possibility of friction or wear.

May be used for a small cable without a messenger.

Applicable to windlasses.

I have described my double capstan, as it is to be used in large vessels, where messengers are necessary, from the great size of the cables; but it is obvious that it is equally applicable in smaller vessels, as their cables can be managed with it in the same manner as is directed for the messenger. The same principle may also be easily applied to windlasses, by having a small horizontal barrel placed parallel to the body of the windlass, and having both fitted with rings, in the same way as the capstan already described. The proper place for the small horizontal barrel is forward, just before the windlass, and as much below its level as circumstances will admit; it should be furnished with catch-palls as well as the windlass.

Farther advantage stated.

Beside the advantages already stated, my proposed improvement to the capstan has others of considerable utility. Its construction is so very simple, that it is no more liable to derangement or injury than the capstan itself. Its cost can be but small, and every part of it can be made by a common ship carpenter, and be repaired by him at sea if damaged by shot. It will take up but little room, only that of a half barrel

Mr. C. Wilson's secure Sailing Boat or Life Boat.

Fig. 1.

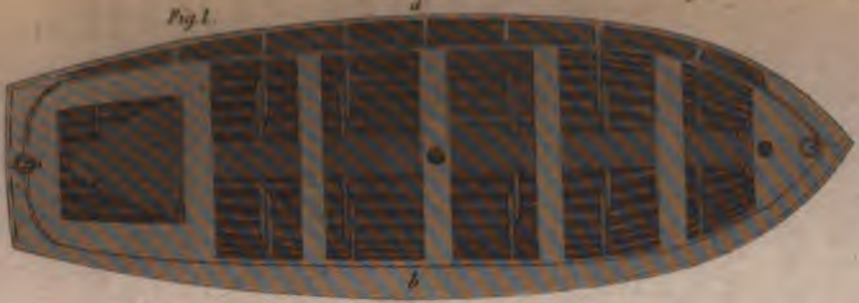


Fig. 2.

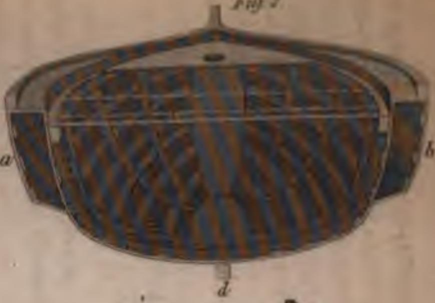
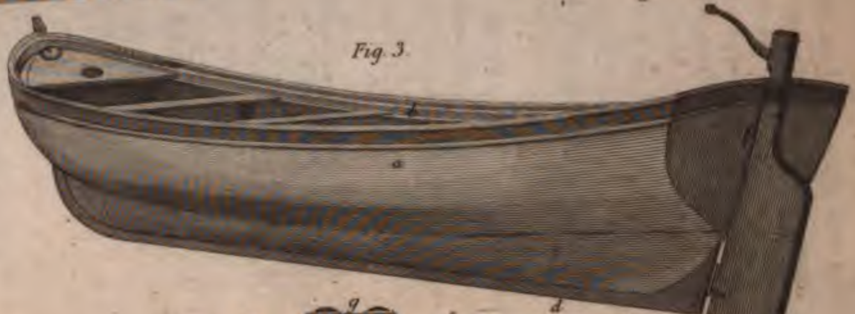


Fig. 3.



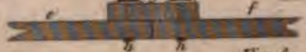
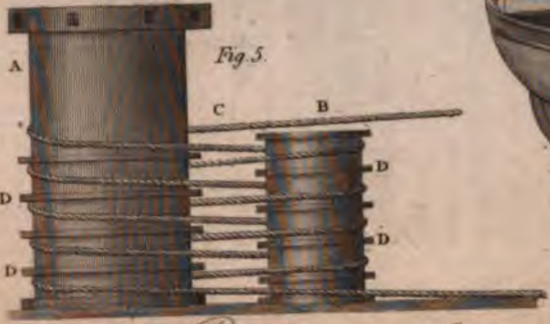


Fig. 4.

Fig. 5.



Mr. J. W. Boswell's improved Capstan.

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TILDEN FOUNDATIONS
R L

barrel cask; and it is of a nature so analogous to that kind of machinery, to which sailors are accustomed, that it can be readily understood and managed by them.

In order to render the description of my double capstan more clear, I annex a sketch of it, as fitted up in the manner proposed.

I am, Sir,
Your very respectful humble servant,
J. WITLEY BOSWELL.

Reference to the Engraving of Mr. Boswell's improved Capstan, to prevent the necessity of surging. Plate 4, Fig. 5.

Explanation of the plate.

A Represents the larger or common capstan used on board ships.

B Another capstan of less dimensions, placed in a similar manner.

C The coils of the messenger passing alternately round the large and small capstans, but with their direction reversed on the different barrels, so that they may cross each other in the interval between them.

D D D D Projecting rings round each capstan or barrel, so fixed on the two barrels, that those on one barrel should be exactly opposite the middle of the intervals between those on the other barrel.

X.

Letter from Dr. BEDDOES *on certain Points of History, relative to the Component Parts of the Alkalis, with observations relating to the Composition of the Bodies termed Simple.*

To Mr. NICHOLSON,

DEAR SIR,

Alkalis not supposed by Dr. B. to be

I Never regarded the base of the alkalis as belonging to the metallic order of combustibles, or projected their reduction metallic, though substances saturated with oxigen.

Arranged with certain earths as a distinct class of bodies.

Query respecting them.

duction by galvanism or electricity. But long ago, on contemplating all other substances in opposition to oxigen it very naturally occurred, that, since alkalis and earths would not burn or absorb oxigen, they might be already saturated with it. This investigation, caused by Tondi's paper, would have been, had it operated at all, a discouragement to the idea, which was certainly formed on different grounds, and existed, I believe, prior to my acquaintance with those facts. Such as it was conceived, it happened to be long afterwards thrown out in an essay on the arrangement of bodies on the principle alluded to above. As a distinct fourth class of bodies I had arranged together barytes, strontites, potash, soda, lime, magnesia, alumine, jargonites, silex, &c., adding this query "Does the mode of union of their elements render them nonoxidable? or have they already oxigen or phosoxigen closely combined?" and again "If future experiments should accomplish the oxidation of any of the bodies of the fourth class, such bodies must be transferred to the third class (termed philoxigenous). Should it be discovered, that oxigen enters into their composition, the terms philoxigenous and misoxigenous must be changed*."

Analysis of bodies termed simple suggested.

Electricity long considered as the proper mean.

The fusible combustibles proper for this.

Tried by Mr. Davy.

A gas liberated, and again absorbed.

I had observed, p. 218, that, "more than mere classification, I had it in view to place under the reader's eye certain probabilities, that might lead to the analysis of different bodies, at present considered as simple." This application of electricity is a project, which has lain on the surface of chemistry for above twenty years. I have taken all opportunities, public and private, of pressing its execution. The bodies I have been accustomed to name as the proper subjects for trial were the fusible combustibles, as sulphur and phosphorus. A gentleman, illustrious for his late success in these researches, some time ago mentioned to me his having made this experiment with galvanism. The result was the liberation of some vapours or gas, which disappeared again before the body congealed. The mode of investigation should, in my opinion, still be prosecuted with a much higher power than has yet been employed.

Metals and other combustibles may be

As an incentive and a clew to experiment (which is the only use of hypothesis) I beg leave to repeat, that *metals*

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* Contributions to phys. and med. Knowledge, p. 223.

and

cannot proceed far in this train of speculation with-
etting the prospect of all nature as consisting of two
nts, oxigen and hidrogen.

espect to heat, light, electricity, galvanism, and mag-
, I see not the smallest reason to regard these as distinct
nces, or other than as powers or influences, if we are
follow Berkeley. We have no right to consider any
ty whatever as essential to matter. We have there-
o criterion of materiality. Yet it appears to me, that
sence of gravitation is a much stronger negative ar-
nt than any positive yet produced: and I have no
but all those who have set themselves to weigh *caloric*,
the notion of its being a separate substance, have
miserably disappointed at the result of their experi-
and that, had the result been opposite, they would
triumphed, and justly, in this proof; for it must have
eceived as decisive. Have not adversaries a right to
te?

Heat, light, electricity, galvanism, magnetism, not proved to be any thing but powers.

Perhaps gravitation the only criterion of matter.

genius of accurate experimental investigation may be
the art of striding from inanimate to living nature;
on afterward one may venture to predict, that other
nces, offering other means of analysis, will be disco-
less extensive probably than heat, and more so than
tism, and constituting the difference between the par-
of matter as they happen to be engaged in one class
npounds or the other. The Archaeus, vital principle,
Hunter's materia vitae diffusa, &c., will perhaps come

We may be on the eve of attaining a knowledge of animate nature.

Other influences, affording other means of analysis, to be discovered.

Anticipations

XI.

Analysis of some metallic Sulphurets. By Mr. GUENIVEAU, *Mine Engineer*[*].

Proportion of the metal in sulphurets constant.

Magnetic and other pyrites.

Some still admit the presence of oxigen.

Various specimens examined.

SEVERAL chemists, particularly Messrs. Proust and Hatchett, have paid attention to metallic sulphurets. The first of these gentlemen has shown, that certain metals, as iron, copper, and lead, combine with sulphur in the metallic state and in a constant proportion. Mr. Hatchett has given an analysis of the magnetic pyrites, which he considers as a sulphuret of iron at a minimum, and that of several common pyrites, in which he finds other principles beside iron and sulphur. The experiments of these two scientific gentlemen however have not impressed conviction on the mind of every chemist; and some appear still to admit the presence of oxigen in sulphurets of iron. They found their objections chiefly on this, that Mr. Proust employed the method of synthesis, which always leaves some uncertainty in the proportions: and that Mr. Hatchett ascertained the sulphur only by means of sulphate of barytes, respecting the composition of which some uncertainty still remains. Having had occasion to analyse certain metallic sulphurets, I determined their elements with a great deal of care, in order to satisfy myself on the points just mentioned.

Sulphuret of iron.

The specimen of sulphuret of iron, on which I made all the experiments I am about to describe, was amorphous, without any mixture of gangue. Its colour was the common bronze yellow of iron pyrites. Various preliminary experiments convinced me, that this mineral contained no earthy substance, and no other metal than iron. I shall now proceed to describe the methods I employed to determine with precision the quantities of iron and sulphur they contained.

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[*] Journal des Mines, vol. XXI, p. 105. A translation of a paper by Mr. Gueniveau on the Desulphuration of Metals, in the same work, was given in our Journal for November last, vol. XVIII, p. 197.

I. *Examination for the iron.*

Analysed. Boiled in aqua regia.

Precipitated by ammonia.

1. I boiled a mixture of nitric and muriatic acids on five grammes [77 grs.] of powdered pyrites. The sulphur was completely burned, and the solution was complete, except 0·01 of a gramme of silex. The oxide of iron precipitated by ammonia and heated red hot weighed 3·35 gr.: which indicate, supposing the proportion to be 148 to 100, 2·25 gr. of metallic iron, or 45 per cent.

This repeated.

2. Another experiment made in the same manner yielded me 3·34 gr. of red oxide of iron; which coincides with the preceding.

Roasted.

3. I roasted 20 gr. [308 grs.] of the same pyrites. After being exposed some hours to a pretty violent heat, the weight was reduced to 13·24 gr.: so that 100 left only 66·2.

Residuum dissolved in aqua regia.

No precipitate with muriate of barytes.

Of this residuum I dissolved 5 gr. in nitromuriatic acid. Muriate of barytes producing no precipitate in this solution, I concluded, that the roasting had been complete, and the pyrites reduced to pure oxide of iron. Besides, on comparing the weight of the residuum of 5 gr. of pyrites, being 3·31 gr., with that of the oxide of iron obtained by the experiment above, namely 3·34 gr., there can be no doubt, but the whole of the sulphur and sulphuric acid were volatilized. This new method of computing the quantity of oxide of iron leaves no doubt respecting the proportion of metal in the pyrites, being equally indicative of 45 per cent of metallic iron.

Roasted pyrites fused without addition.

4. I fused the roasted pyrites without any addition in a crucible lined with charcoal, in order to obtain the metal. The button amounted to 70·2 per cent, without any scoriæ. Deducting 3 per cent for the carbon combined with it, we shall have 68·1 of iron from 100 of roasted pyrites, and from 100 of pyrites in its native state 45·08 of pure iron.

Contained 0·45 of metallic iron.

From the four experiments here mentioned it follows, that the sulphuret of iron contains 45 hundredths of metallic iron; and I do not think, that any errour can have taken place to the amount of one hundredth.

II. *Examination*

II. *Examination for the sulphur.*

Dissolved in aqua regia and precipitated by muriate of barytes to ascertain the quantity of sulphur.

1. Having dissolved 5 gr. of iron pyrites in nitromuriatic acid with the assistance of heat, I dropped into the solution muriate of barytes, till no more precipitate was formed. The sulphate of barytes subsided to the bottom of the vessel; and, having poured off the clear liquor, I added some distilled water, in order to wash off any foreign salts. I collected the sulphate on a filter. Having dried it, first with a gentle heat, increased afterward to redness, and burned the filter separately, I found the weight of the sulphate of barytes, deducting that of the ashes of the filter, was 19·1 gr., or 382 to 100 of pyrites.

2. It might be suspected, that the preceding result was too small, on account of the state of ebullition in which I had kept the solvent, which might have carried off in vapour a portion of the sulphuric acid formed. I thought it right therefore, to make another experiment, employing a more moderate heat.

Treated with dilute nitric acid in a gentle heat.

Accordingly I treated 2·5 gr. of the same pyrites with diluted nitric acid, heating it gently. The whole of the sulphur however was burned except about 0·03 of a gramme that remained undecomposed. From this solution I obtained 9·71 gr., or 388 per cent of sulphur of barytes, corresponding to 54·3 of sulphur; and, on taking into the account the residuum abovementioned, we shall have 54·8 of sulphur in a hundred parts.

This result I consider as more accurate than the preceding.

0·55 of sulphur.

The experiments I have related clearly show, that the sulphuret of iron analysed contained about 45 per cent of metallic iron, and between 54 and 55 per cent of sulphur, results which differ very little from those of Mr. Hatchett. It is difficult then to conceive, that iron pyrites contain oxigen, and the quantity corresponding to all the mistakes that could possibly have taken place cannot be many hundredths.

Component parts of iron pyrites.

Metallic iron	45
Sulphur	55
	100.

Sulphuret of copper.

Sulphuret of copper.

Messrs. Lelievre and Gillet-Laumont, mine-counsellors, having had the goodness each to present me with a specimen of sulphuret of copper, I shall proceed to give the results of my analysis of this mineral.

1. Siberian sulphuret of copper from the collection of Mr. Lelievre. Spec. gravity 5·22.

Treated with aqua regia.

Five grammes of this mineral treated with nitromuriatic acid assisted by heat were reduced to 0·51 of a gr. of sulphur nearly pure. Calcination left only 0·04 gr. of oxide, which was completely redissolved.

Precipitated by muriate of barytes.

The solution precipitated by muriate of barytes let fall 4·01 gr. of sulphate, corresponding to 0·56 of a gr. of sulphur. This brings the whole quantity of sulphur to 1·03 gr. The iron was separated from the copper by ammonia. The precipitate, well washed and dried, weighed 0·08 of a gr.

The brown oxide of copper precipitated by potash weighed 4·65 gr., answering to 3·72 gr. of metallic copper.

No earth, lead, manganese, or antimony.

A little oxide of iron foreign to it.

I convinced myself by various experiments, that the specimen subjected to analysis contained no earthy substance, or lead, manganese, or antimony. The small quantity of iron existing in it appeared to me even to be included in small fissures, in which its oxide is easy to be perceived: it cannot therefore be considered as an essential part of the composition of sulphuret of copper.

The results of this analysis are

Component parts.

Metallic copper	74·5
Sulphur	20·5
Oxide of iron	1·5
Loss	3·5
	100.

The experiments I made in the course of the analysis lead me to think, that part of the loss fell on the copper. Notwithstanding this however, the proportion of sulphur to copper differs very little from that of 28 to 100 given by Mr. Proust.

Fused in charcoal.

This sulphuret of copper, being subjected to a very violent fire in a crucible lined with charcoal, was fused, and lost but 2½ per cent of its weight. Its aspect was not altered, only a few small globules of copper were perceptible toward the bottom of the button.

2. *Siberian sulphuret of copper from the collection of Mr. Gillet-Laumont.*

Another specimen mixed with quartz.

This specimen, though in appearance very homogeneous, was notwithstanding mixed with a great deal of quartz. In some places it struck fire with steel.

Analysis.

I separated the copper from the iron by sulphuretted hydrogen. The precipitate, calcined, redissolved, and treated with caustic potash gave me 3 gr. of oxide of copper from 5 of the mineral. I found in it no other metal but copper and iron.

The results were

Component parts.

Metallic copper	47
Sulphur	13
Siliceous residuum	25
Lime	7
Red oxide of iron	9·3
	101·3

Proportions of the sulphur & metal not affected by foreign substances.

It is to be observed here, that the presence of the different substances foreign to the sulphuret of copper did not affect the proportions of the copper to the sulphur, which is evidently that of 100 to 28. The iron probably is not combined with the sulphuret, but with the silex and lime forms its gangue.

Copper pyrites.

Pyritous copper.

I. Copper pyrites of Sainbel from the collection of the Council of Mines. Spec. gravity 4·16.

The specimen I subjected to analysis was amorphous, but without mixture of gangue. Its colour was a greenish yellow bronze. I ascertained its composition in two different methods.

1st

Treated with aqua regia.

1st analysis. Five grammes of this mineral, powdered, and treated with nitromuriatic acid, were very easily attacked by it. The residuum, weighing 1·13 gr., was reduced to 0.08 of a gr. by calcination; and an addition of fresh acid left only 0·04 of a gr. of quartzose gangue.

Precipitated by muriate of barytes, and then by sulphuretted hidrogen.

Muriate of barytes threw down from the solution a precipitate of sulphate weighing 5·5 gr., corresponding to 0·77 of a gr. of sulphur. This quantity, added to that already formed, gives 1·82 gr. for the whole of the sulphur it contained. The copper was precipitated by sulphuretted hidrogen, redissolved, and precipitated afresh by caustic potash. The brown oxide obtained weighed 1·88 gr., and contained nearly 1·5 gr. of metal.

The potash had dissolved about 0·05 of a gr. of oxide of zinc. The red oxide of iron weighed 2·26 gr., corresponding to 1·53 of metallic iron.

Results.

Component parts.

Sulphur	36·5
Copper	30
Metallic iron	31
Oxide of zinc	1
Gangue	1
	99·5

Treated with nitric acid.

2d analysis. The same substance was treated with nitric acid assisted by heat.

Residuum calcined and treated with aqua regia.

First residuum, 2·35 gr. reduced to 1·86 gr. by calcination. The nitromuriatic acid left of this only 0·23 of a gr., containing only 0·04 of a gr. of gangue.

Precipitated by muriate of barytes.

The weight of the sulphur separated from these residuums was 0·93 of a gr. The solution gave a precipitate of 5·91 of sulphate of barytes, containing 0·82 of a gr. of sulphur, and making the whole amount to 1·75 gr.

Copper dissolved by ammonia, and iron separated.

The copper was dissolved by ammonia, and the oxide of iron separated from it by several operations. The oxide of copper precipitated by potash weighed 1·9 gr., answering to 1·52 gr. of the metal.

The red oxide of iron weighed 2·47 gr., and contained 1·66 gr. of pure iron.

I likewise found traces of zinc.

Result.

Component parts.

Sulphur	35
Copper	30·5
Metallic iron	33
Some traces of zinc	
Gangue	1
	99·5

If we take a mean of the results of these two analyses, we shall have as very probable proportions,

Mean of the two analyses.

Sulphur	36
Copper	30
Metallic iron	32
Gangue	1
Zinc	1
	100

Another specimen.

II. Copper pyrites of Baigorry.

For the two following analyses I employed pieces of ore that were sufficiently pure, though mixed with quartz.

Treated with aqua regia.

1st analysis. Five grammes reduced to powder were subjected to the action of nitromuriatic acid. The first residuum, weighing 1.72 gr., was reduced to 0·73 of a gr. by calcination. An addition of acid left only 0·54 of a gr., and of these 0·46 were found to be gangue, after the sulphur had been burned.

Precipitated by muriate of barytes,

The muriate of barytes precipitated from the solution 3·6 gr. of sulphate, corresponding to 0·5 of a gr. of sulphur. The whole of the sulphur therefore was 1·57 gr.

and sulphuretted hidrogen.

Sulphuretted hidrogen was employed to separate the copper. The brown oxide of this metal, precipitated by potash, weighed 1·69 gr., and consequently contained 1·35 of metal. The red oxide of iron obtained weighed 2·19 gr., corresponding to 1·49 gr. of metallic iron,

Result.

Component parts.

Sulphur	31·5
Copper	27
Metallic iron	30
Gangue	8·5
	97

Analysis repeated.

2d analysis. I treated 5 gr. of the same substance in the same way. I separated by calcination 0·34 of a gr. of sulphur. The gangue weighed 0·48 of a gr. The sulphate of barytes obtained weighed 8·88 gr., corresponding to 1·24 gr. of sulphur. The whole of the sulphur therefore was 1·58 gr.

The brown oxide of copper weighed 1·73 gr.; the red oxide of iron, 2·16 gr.

Component parts.

Result.

Sulphur	31·5
Copper	28
Metallic iron	29
Gangue	9
	97·5

Mean of the two analyses.

Mean proportions.

Sulphur	31·5
Copper	27·5
Metallic iron	29·5
Gangue	9
	97·5

Sulphur not completely ascertained,

I have reason to think, that the proportions of sulphur are rather too small, because all the methods employed never give the whole of this combustible.

and generally comes out less than the truth.

When metallic sulphurets are treated with nitric acid diluted in water, the sulphur remains mixed with the metals, which become oxided during the evaporation. All the oxigen added diminishes the quantity of the sulphur. By employing nitromuriatic acid and boiling, this inconvenience is avoided; but sulphuric acid may be carried of in vapour. Whatever method we adopt, the quantity of sulphur obtained may always be considered as below what really exists.

Proportions of sulphur, copper, and iron nearly uniform.

Notwithstanding the errours unavoidable in analyses, it is easy to perceive, that the relative quantities of sulphur, copper, and iron, are nearly the same in the two specimens of copper pyrites. Setting aside the gangue, and reducing the proportions to hundredth parts of pure ore, we find

	Sulphur.	Copper.	Iron.
In the copper pyrites of Sainbel	37	30.2	32·3
In that of Baigorry	35	30·5	33

A mixture of the two sulphurets.

Mr. Proust has shown, that the copper pyrites contains sulphuret of copper completely formed, and he considers this mineral as a mixture of the two sulphurets of copper and iron. This opinion appears to me very probable: though perhaps we have not sufficient grounds to assert, that the sulphuret of iron exists in it in the same state of combination as that, which constitutes native iron pyrites.

Analysis of Mr. Chenevix nearly similar.

Mr. Chenevix obtained from a specimen of copper pyrites 30 per cent of copper, and 53 of oxide of iron, corresponding to 35 of the metal. I likewise found 30 per cent of copper in a piece of yellow ore from Chessy. On comparing these results with those above given, I was struck with the proportion that exists between the elements of a mineral generally considered as varying greatly in its composition. The difficulty of distinguishing it from iron pyrites may have contributed to this opinion: but I am inclined to think, that, when copper pyrites is completely homogeneous, and not decomposed, its composition is the same, from whatever ore it be obtained; and that it may be considered as a mineralogical species, ascertained and determined by chemistry.

This confirms the idea of its uniformity.

This however is but a simple conjecture, on which nothing positive can be said, till we have a greater number of analyses made on well marked specimens free from any mixture.

XII.

Analysis of a Carbonate of Lime from Pesey; *by Mr.* BERTHIER, *Mine Engineer*[*].

The carbonate described.

THE carbonate of lime from the mine of Pesey is found in geodes grouped comfusedly with quartz, and sometimes with lenticular polishing spar. Its specific gravity is 2·97.

Its figure is that of the primitive rhomboid of common carbonate of lime. It may be split with great facility, and divides in the direction of its longer diagonals. All the faces are covered with striæ in this direction.

Its hardness is much greater than that of common carbonate of lime, which it scratches. It even scratches arragonite. The pieces found on the heaps of rubbish, that have remained long exposed to the air, have the brown colour of iron spar. It is sometimes perfectly transparent: but most commonly it has a slight yellowish tint, becomes opake, and is covered with a brown oxide, as it is decomposed in the damp parts of the mine.

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[*] Annales de Chimie, vol. LVIII, p. 87.

Before the blowpipe it becomes black, and is slightly altered. It scarcely effervesces with acids, unless previously powdered.

Analysed. Treated with nitric acid.

Having powdered and sifted it, I took 5 grammes [77 grains], on which I poured strong nitric acid. On applying a gentle heat, the effervescence immediately became very brisk; nitrous gas was evolved; and the powder assumed a brown colour. Having evaporated to dryness, I poured on fresh acid, and repeated the same operation.

With muriatic, & precipitated by prussiate of potash,

I next dissolved the whole in muriatic acid, evaporated gently to expel the excess of acid, and then dissolved in water. The solution, which was of a light yellow, was precipitated by prussiate of potash. The result was a deep blue prussiate, which was filtered and washed.

and by oxalate of ammonia.

The solution, completely neutralized, was precipitated by oxalate of ammonia, and yielded oxalate of lime, which, when washed and dried, weighed 3·95 grammes.

Caustic potash threw down magnesia.

Caustic potash threw down from the liquor a copious white flocculent precipitate, which, when washed, dried, and calcined in a red heat, weighed 0·5 of a gr. This substance, which was of a fine white colour, dissolved entirely in sulphuric acid, and yielded a bitter salt. Carbonate of ammonia did not precipitate it; therefore it was magnesia.

Iron oxided,

The prussiates when dried were strongly calcined, and the residuum oxided to a maximum by nitric acid.

dissolved in muriatic acid,

The oxides were redissolved in muriatic acid; evaporated gently to neutralize them; and then diluted with a large quantity of water. No residuum was left.

precipitated by potash,

This solution was precipitated by carbonate of potash, saturated, and afterward filtered.

redissolved and precipitated.

The carbonate deposited on the filter was redissolved in muriatic acid, precipitated afresh by carbonate of potash, saturated, and filtered.

Liquors evaporated

The two filtered liquors being mixed together, they were subjected to evaporation for about two hours, when they deposited a slightly yellowish white substance, which, when washed and dried, weighed 0·16. Before the blowpipe it immediately became black, and communicated a violet colour to borax. In nitric acid it redissolved with effervescence; the nitrate grew black on drying; and the residuum, treated with muriatic acid, gave out oxigenized muriatic acid, and produced a brown colour, which heat removed. Lastly, prussiate of potash threw down from the muriatic solution a white precipitate, without any perceptible mixture of blue. Thus there can be no doubt, that the carbonate deposited by ebullition was carbonate of manganese.

deposited manganese.

Iron left on the filter.

The supernatant liquor contained nothing more. The carbonate remaining on the filter was red. This was dissolved in muriatic acid, and precipitated by prussiate of potash, which produced a blue prussiate of iron, weighing, when well washed and dried, 1·9.

The carbonate calcined.

Five grammes of the carbonate were calcined in a crucible at a red heat, and lost from 1·8 to 1·85 of water and carbonic acid.

This substance therefore contained, in 100 parts,

Component parts.

Lime	43·5
Magnesia	10
Black oxide of iron	8
White oxide of manganese	2
Water and carbonic acid	36·5
	100.

A compound of four carbonates.

Thus the four carbonates of lime, magnesia, iron, and manganese, which sometimes occur separately in nature, are found united together in the substance, of which an analysis has just been given. I do not think there can be any doubt of the presence of the manganese. In one experiment I found four per cent of this metal in the state of white oxide: but I have preferred giving the proportion above, which consequently is a minimum.

These carbonates variously compounded.

If the results here related be exact, we may conclude, that the carbonates of lime, magnesia, iron, and manganese, may be found in nature in various proportions; so that it is no wonder we meet with iron spars without manganese, and others mixed with manganese alone, without lime or magnesia.

nesia. The analyses of these substances become so much the more interesting to the metallurgist, and we see clearly one source of the difference, that may exist between iron spars.

XIII.

Chemical Examination of the Stalk of Indian Corn, Zea Mays Lin., to ascertain whether the Saccharine Matter it contains be capable of Crystallization: by Mr. V. AUARIE, Apothecary at Valence, Department of the Drôme.*

Analysis of vegetables far from perfect.

IF it can be said with truth, that our physical knowledge of vegetables is a complete science, their analysis individually is far from having attained this desirable end. The labours of modern chemists however are paving the way for it; their numerous scientific discoveries have already illustrated this subject, of so much importance to the art of pharmacy; and other arts, as well as that of physic, are daily availing themselves of it with success. Still we have to regret, that the analysis of vegetables is most uncertain, since the results are too often far from satisfactory, and the synthetical method is in many cases impracticable.

Stalks of Indian corn boiled in water.

I. I boiled in a sufficient quantity of water fifteen pounds of the stalks of Indian corn, freed from their leaves and roots, and previously bruised. The decoction after it was filtered was of a golden yellow colour, and a saccharine taste. Part of this decoction was subjected to various experiments, of which the following were the results.

The decoction tested with

acetite of lead,

1. A solution of crystallized acetite of lead rendered the decoction turbid, and separated its colouring and extractive part. These had subsided to the bottom of the vessel in the course of an hour, leaving the liquor very clear, and lighter coloured.

oxalate of potash,

2. The acidulous oxalate of potash produced a sediment, and left the liquor milky.

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* Annales de Chimie, vol. LX, p. 61. Some remarks on the charcoal both of the stalks and seeds of maize, by prof. Proust, were inserted in our Journal, vol. XVIII, p. 239.

acetite of ammonia,

3. A solution of acetite of ammonia heightened its colour, and produced a scarcely perceptible precipitate.

muriatic and oximuriatic acid,

4. Common muriatic acid occasioned no change: but oxigenized muriatic acid produced a slight precipitate, without altering the colour of the liquor. This precipitate was occasioned by the oxigen, which attacked the extractive matter, and rendered it insoluble in water.

potash,

5. Caustic potash produced no change.

lime-water,

6. Lime-water and prussiate of lime rendered it slightly turbid.

sulphate of iron,

7. Sulphate of iron had no effect on it, not even altering its transparency.

sulphuret of potash,

8. It was the same with sulphuret of potash.

nitrate of mercury,

9. Nitrate of mercury was decomposed, and formed a coagulum, which subsided to the bottom of the vessel and was of a deep gray colour.

and alcohol.

10. Alcohol produced no satisfactory result.

Liquor still saccharine.

It is to be observed, that none of the reagents employed either destroyed or altered the saccharine taste, which continued in the liquor; nothing being decomposed and precipitated but the gummy extract.

Decoction evaporated & left at rest, but no crystals.

II. The greater part of the decoction was evaporated to the consistence of a sirup, and afterward set by. Having been left undisturbed for twenty days at a temperature of 10° [50° F.], it was just the same, without any appearance of crystals or sediment.

Diluted, clarified, and again left at rest. No crystals.

III. Having diluted it with twenty times its bulk of water, clarified it afresh, to separate the mucilage that appeared to prevent its crystallization, and reduced it to a syrupy consistence, I was not more successful, after leaving it a proper time at rest.

Evaporated & digested in alcohol.

It was of importance therefore, to separate the extractive matter from the saccharine part. Accordingly I evaporated the decoction, thus clarified a second time, to the consistence of an extract, and digested it in a sufficient quantity of alcohol. Twenty-four hours after I filtered it, when the alcohol had dissolved half the matter subjected to its action.

Solution.

The alcoholic solution had the taste of a very sweet dram, except that it had no aromatic flavour. Its colour was a brown yellow.

This solution mixed with water underwent no change, remaining perfectly clear; which is a proof, that the alcohol had dissolved no resin, and that the saccharine matter only was in the solution. No resin.

After having separated the alcohol in the common way, the substance remained fixed, and would not crystallize. It comported itself like the treacle of the shops. Not crysallizable.

Not being by any means satisfied with the results above mentioned, I again diluted the sirupy matter, that had been dissolved by the alcohol, with a sufficient quantity of water; I added a little lime and white of egg in the clarification; and after filtering and evaporating to a due degree I set it by for two months in a stove, but without obtaining any crystals. Clarified with lime and albumen. No crystals.

I employed successively all the processes employed in sugar-houses, without any success. I carried my experiments so far as to boil it with charcoal, and after I had clarified it, I was equally unsuccessful. It retained, and still retains, for I have left it to the effect of time, its honeylike appearance; yet it possesses all the other characters of the true sugar extracted from the sugar-cane of the West Indies. Various methods tried without success.

IV. The substance that remained insoluble in the alcohol was completely dissolved in distilled water. Its taste was saponaceous and slightly saccharine. After evaporating to the consistence of an extract, it weighed four ounces and half. One ounce of this was treated with nitric acid, which dissolved it in the same manner as it would have done a gum. During the solution a great deal of nitrous gas was evolved, and at the same time oxalic acid was formed. Matter insoluble in alcohol. Treated with nitric acid-comported itself as gum.

The remaining three ounces and half of extract were afterward incinerated. During the combustion a large quantity of carbonic acid was given out; the matter swelled up, so that the coal was twenty times the original bulk, and very porous; the residuum, after incineration, weighed half an ounce; and this, when dissolved, filtered, and evaporated, was reduced to two drams. Incinerated.

I found by the processes I employed, that the salt resulting from these operations was carbonate of potash with a little magnesia. Carbonate of potash and a little magnesia.

From all that has been said above it follows:

1st, That the stalk of Indian corn cannot be employed for General conclusions.

for the extraction of sugar, because the expense would exceed the profit, since a hundred weight yields only about two pounds of saccharine matter.

2d, That the saccharine matter constantly retains the consistence of treacle, and is incapable of being crystallized by any known process.

3d, That the gummy extract might be employed in medicine as an attenuant, in consequence of its saponaceous quality.

XIV.

On the Culture of Spring Wheat, and the Use of Tincture of Opium in the Diseases of Cattle: by Major SPENCER COCHRANE, *of Muirfield-Houſe, near Haddington, North-Britain*[*].

SIR,

I REQUEST the favour that you will preſent my thanks to the Society of Arts, &c. for transmitting me the 20th volume of their Transactions, containing my former experiments on the culture of wheat sown in the spring. I have since had further proofs of the advantage resulting from that practice.

Advantages of spring wheat.

Notwithstanding the extreme cold weather, which we had here during the months of March, April, and May, I never saw my spring wheat look better, particularly four acres, part of a field of strong clay, which I was prevented from sowing in October, when I sowed my other wheat, by wet weather commencing.

The whole field consists of twenty acres, and received one ploughing after drilled beans. I sowed on the 14th of March the four acres, with four bolls or two quarters of common wheat, on the same earth or furrow which the land got in the month of October.

Should be sown on winter furrow.

In the event of land having been fallowed and sufficiently cleaned before winter, and wet weather setting in so as to prevent wheat being sown at the usual time, I recommend from experience, that the wheat be sown in the spring on the winter

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[*] Trans. of Soc. of Arts, vol. xxv. p. 29. The silver medal of the Society was voted to Major Cochrane.

ter furrow, and that it should by no means be ploughed again in the spring.

The winter frost meliorates the soil, and I think kills the annual weeds. I have remarked that by adopting this mode, the land is much less troubled with them, the weeds having been a general objection to spring wheat.

If spring wheat follows turnips, the ground ſhould be ploughed as soon as possible, if the soil is of a wet nature, to correct the injury the land may have sustained by leading off the crop, and by the poaching of carts and horses. Frost will in some degree correct what should never if possible happen, *wet ploughing*.

Proper time for sowing.

From the middle of February till the 10th of March is the proper time for sowing wheat in the spring, provided the land is sufficiently dry. Then on the first furrow let the seed properly pickled be sown either by drill or broad cast; the usual practice of water furrowing, to keep the land from too much rain, being properly attended to.

On the Uſe of Tar for Cattle swelled by eating Clover.

Tar cures cattle swelled with eating clover.

Cows are frequently seized with violent swellings from having been imprudently allowed to eat clover when wet. A gentleman recommended to me, as a cure, an egg-shell full of tar, immediately to be put down the creature's throat. In two instances of my own cattle I found it had the effect of laying the swelling in a few minutes. A neighbour of mine, whose cow it was supposed could not live five minutes, was, on application of the tar, unexpectedly recovered, to the great joy of the poor man.

On Opium and other Preparations from Poppies.

Opium a cure for the gripes in horses.

After I commenced farmer, I unfortunately loſt four horses, by a disorder very frequent in this country, called the *bats* or *gripes*; some of them died in a few hours, and none of them were ill more than two days. For some years past I have given my horses in such cases a table spoonful of tincture of opium, or liquid laudanum, and have since lost none. If the firſt dose given in some liquid does not allay the violent pain and swelling, I administer a second spoonful, which I have hitherto, in all cases, found to have the desired effect, and generally in a very short time.

If I find the horse very hot and feveriſh, and sweating profusely, as is usually the case in this disease, I order him to be bled plentifully, and an ounce or more of nitre to be mixed, and administered with the laudanum, keeping the horse warm, and letting him be well rubbed round the belly.

A very considerable farmer near me, who has had a medical education, told me, a few days ago, that he had not lost a horse since he gave them laudanum.

Equally useful to sheep, with common salt.

Ten days ago I was equally fortunate in a trial of it on one of my sheep, which, half an hour after being washed with the rest of the flock, was taken so extremely ill, and swelled so much, that my herdsman supposed she could not live, having lost some of his own, which had apparently been in the same state. I immediately ordered half a handful of common salt to be dissolved in half an English pint of warm water, into which I put sixty drops of the laudanum, and poured it with difficulty down the animal's throat, which seemed nearly dead. For the first five minutes I had so little hopes of the sheep's recovery, that I ordered the man to get his knife ready to cut her throat; whilſt he sharpened the instrument for such purpose, he observed the animal to move his jaw to a proper position, which had previously been much distorted; the eyes then began to quicken, and apparently to become at ease. In half an hour afterwards the sheep got on her legs, and remained standing for some time; a plentiful evacuation soon took place, the swelling subsided, she continued to recover, and in a few hours from the first attack began to eat and do well.

My intent in these communications is to render generally public what I have found so very beneficial. At this time, when horses and cattle are so extremely high in price, every thing that can tend to preserve their lives, ſhould be made known and put to trial.

Poppies cultivated for the opium.

I formerly noticed to you, that I had tried on a small scale, for several years, the culture of white poppies to prepare opium from them, and an extract or syrup of poppies: that I had raised a sufficient supply for myself and friends, and that my extract was equal in effect to any prepared from foreign opium. I recommend the poppy seeds to be sown in March, in drills.

and for oil.

Beside the advantages from the poppy heads as a medicine, the seeds yield a valuable oil. Two pounds of the seeds furnish by expression seven ounces of a pure bland oil, useful for portrait painting and other purposes. It has been proved in Holland to be equal in quality to fine salad or olive oil, and it would probably be advantageous to propagate largely so valuable a plant*.

I am, Sir, your humble servant,
SPENCER COCHRANE.

SCIENTIFIC NEWS.

Decomposition of the Earths.

Metals obtained from most of the earths.

IN a paper lately read before the Royal Society Mr. Davy has detailed a number of experiments, made by means of Voltaic electricity, on the common and alkaline earths, by which he has succeeded in effecting their decomposition, and obtaining metals from most of these refractory bodies.

Revived in alloys by mixing with metallic oxides.

His method of decomposing the alkaline earths is by electrifying mixtures of them and metallic oxides, such as those of quicksilver, silver, and tin. The common metals and the metals of the earths are revived together in alloy.

Bases of barytes & strontites.

Lime and magnesia.

He has succeeded in obtaining the pure metals of barytes and strontites, by distilling their amalgams: and in the same way has procured the metals of limé and of magnesia nearly pure.

Alumine and silex.

He has obtained marks of the decomposition of alumine and silex, by electrifying mixtures of these earths and potash, but has not yet succeeded in obtaining their metals pure.

Hidrogen and nitrogen form an amalgam with mercury.

Mr. Davy has repeated a remarkable experiment of Messrs. Berzelius and Pontin, of Stockholm, from which it appears, that hidrogen and nitrogen are capable of combining with quicksilver, and of forming with it a metallic amalgam, which by oxidation produces ammonia.

Lectures on the nature, use, & properties of air.

Mr. George Singer will commence his lectures at the Scientific Institution, No. 3, Princes Street, Cavendish Square, early in November, with an extensive course, on the nature, use, and properties, of the atmosphere, an historical sketch of the progress of atmospherical discovery, and an experimental elucidation of every interesting phenomenon dependent on the agency of air, including pneumatics, hydrostatics, natural chemistry, and meteorology, illustrated by an extensive and appropriate apparatus.

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* On this subject see our Journal, vol. XIX, p. 282.

METEOROLOGICAL JOURNAL

For SEPTEMBER 1808,

Kept by **ROBERT BANCKS**, Mathematical Instrument Maker, in the STRAND, LONDON.

AUG. Day of	THERMOMETER.				BAROMETER.	WEATHER.	
	9 A. M.	9 P. M.	Highest.	Lowest.		Night.	Day.
27	61	61	71	52	29·63	Fair	Fair
28	52	57	67	54	29·74	Ditto	Ditto
29	59	62	69	60	29·93	Ditto	Ditto
30	57	66	72	60	29·77	Ditto	Rain
31	63	60	68	59	29·64	Ditto	Ditto
SEPT.							
1	62	59	67	55	29·66	Cloudy	Rain
2	61	56	65	55	29·84	Ditto	Ditto
3	58	56	65	49	29·86	Fair	Ditto
4	62	57	64	53	29·83	Ditto	Ditto
5	61	57	64	55	29·80	Ditto	Ditto
6	59	58	63	54	29·77	Rain	Ditto
7	58	60	64	57	29·82	Ditto	Ditto
8	61	57	66	55	29·64	Fair	Ditto
9	57	59	62	54	29·38	Rain	Ditto
10	56	57	60	53	29·37	Fair*	Ditto
11	56	56	60	54	29·51	Ditto	Ditto †
12	55	56	59	54	29·66	Cloudy	Ditto
13	56	58	61	51	29·74	Ditto	Ditto
14	57	58	63	57	29·82	Ditto	Fair
15	62	60	68	56	30·05	Fair	Ditto
16	61	57	65	52	30·26	Ditto	Ditto
17	60	58	64	54	30·22	Ditto	Ditto
18	59	61	66	58	29·98	Ditto	Rain
19	61	59	65	53	29·99	Ditto	Ditto
20	58	58	66	50	30·23	Ditto	Fair
21	57	57	64	52	30·31	Ditto	Ditto
22	56	60	68	52	30·15	Cloudy	Rain
23	56	50	64	44	29·71	Fair	Ditto
24	48	51	54	48	29·99	Ditto	Fair
25	52	54	57	49	30·08	Ditto	Ditto

* Foggy at midnight.

† Thunder at 11 A. M.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1808.

ARTICLE I.

Observations on the Exhausting Machine of Dr. Thomas Stewart Traill, *by Mr.* Robert Bancks, *Mathematical Instrument Maker, in the Strand.*

To Mr. NICHOLSON.

SIR,

The same things thought of by diffeient persons.

In the present enlightened state of science, it is not to be wondered, that different scientific men should have similar ideas, with the hopes of constructing instruments in the utmost state of perfection. I am led to this reflection by a description of a machine, in your Journal for last month, nearly similar in construction to one made by me ten years ago for Bracey Clark, Esq., a gentleman well known in the scientific world.

Air pump.

After some ingenious observations by Dr. Traill, on the impossibility of obtaining a perfect vacuum in the air pump of ordinary construction, he says, p. 63, "it occurred to me, that, if there was a convenient method of using the Torricellian vacuum, it would be preferable to the common air pump, even when best constructed. After various attempts, the annexed figure and description will give an idea

Torricellian vacuum suggested for it.

of the machine, which I *concieve* well adapted to answer the end proposed."

Plausible in theory.

With all deference to Dr. Traill, I must suppose his ideas are theoretical; and they certainly appear, to those who have not tried such experiment, admirably calculated to produce a perfect vacuum on the Torricellian principle: but I am inclined to think, that the larger an apparatus of this kind is, the more imperfect it must be; since every experienced artist well knows what extreme care is requisite in making barometers of the best construction, where we are obliged to boil the mercury to exclude air and vapour; and as the same means cannot be applied to the instrument alluded to, it follows in consequence, that the air cannot be completely excluded, by the simple manipulation of filling with mercury, and permitting its descent.

The larger the apparatus probably the less perfect the vacuum.

Defect in the cover.

After Dr. Traill has described the instrument, he directs the whole to be filled with mercury, and its cover then to be placed on. Now I believe it is well known from the laws of repulsion, that it will be impossible to fill the receiver in such a manner, as to admit the cover to be put into its proper place, and at the same time exclude all the air necessary for a perfect exhaustion, or *vacuum*. The state of this vacuum too cannot be determined, without a gauge, for which there is no provision in the instrument described.

No gauge.

An apparatus similar to Dr. Traill's.

As I have intruded thus far, I beg to say, that, in the early part of this year, I made a similar apparatus for J. G. Children, Esq. F. R. S. as contrived by himself; and as it appears to have a decided advantage over that of Dr. Traill, I trust a brief description of it may be admitted.

Description of the parts that differ.

It may be sufficient to say, every part is similar in principle to Dr. Traill's, but the recipient of small capacity, and the cover marked B admirably constructed to remove every objection as to the complete filling with mercury. Let fig. A therefore represent the recipient; B the cap, the under side conical as shown; C, a cock with a funnel; D, another cock on the opposite side of the cap. Now after the receiver was filled as high as convenient, the cap (or stopper) was put into the neck. Mercury was then poured into C through the funnel, until it passed out at D (according to the law of fluids finding its level), when both the cocks being turned, communication with the atmosphere was cut off. The cock was then opened at the bottom of the tube E, the mercury descended, and some sort of vacuum was produced.

State of the vacuum tried with a gauge.

In order to see the state of this vacuum, as we had an iron tube, I suggested a gauge, which was easily introduced before filling, and floated on the surface of the mercury within the tube. The index of this gauge, passing into the vacuum, exhibited the height of the mercury. But I must confess, though every part was perfectly tight, and I believe well made, the mercury well dried, and the machine much agitated in filling, to dislodge the air, it did not afford the satisfaction sought after. From this failure I am inclined to think, that so good a vacuum cannot be obtained by such a mercurial apparatus, as with a pump of the best construction, that will indicate an exhaustion to the $\frac{1}{50}$ of an inch with the nicest test, a siphon gauge.

Not equal to that of a good air pump.

Should you think these observations worthy a place in your Journal, they are at your service.

I am,
Your very humble servant,

Sept. 9, 1808. R. BANCKS.

P. S. Could Dr. Traill's instrument be made perfect, and to supersede the use of our best pumps in nice experiments, many of those that may be deemed of importance must be laid aside, from the expense that would be incurred, as the apparatus could be made only in iron or steel, and would come much more expensive than it does at present.

Expensive.

ANNOTATION.

The vacuum not perfect.

IN giving Dr. Traill's instrument, I never had an idea, that a perfect vacuum would be obtained by it. The great difficulty of freeing the mercury from air, it is probable, must ever prove an insuperable obstacle to our complete success. I think farther it is very questionable, whether the contact between the mercury and the sides of the tube would be so complete, as perfectly to prevent any air from insinuating itself between them. But the grand utility of the machine

invented

But it is accomplished at once, and with facility.

invented by Dr. Traill, as well as of those previously invented unknown to him, and here mentioned by Mr. Bancks, appears to me to consist in the being able by its means to exhaust the receiver in a very considerable degree, at a single operation, and without any labour. This great saving of time may be an object of importance on some occasions; as I conceive the exhaustion would be considerable, though not complete, or even equal to that of an air pump of the best kind: for nothing in the remarks of Mr. Bancks contradicts this, and the opinion of Dr. Traill is not merely theoretical, since he rests it on his own experience, though he had not the advantage of an able artist.

II.

On Superacid and Subacid Salts. By WILLIAM HYDE WOLLASTON. *M. D. Sec. R. S*[*].

Superoxalates have a double portion of acid.

IN the paper which has just been read to the Society[†], Dr. Thomson has remarked, that oxalic acid unites to strontian as well as to potash in two different proportions, and that the quantity of acid combined with each of these bases in their superoxalates is just double of that which is saturated by the same quantity of base in their neutral compounds[†].

The same law holds in other instances.

As I had observed the same law to prevail in various other instances of superacid and subacid salts, I thought it not unlikely, that this law might obtain generally in such compounds, and it was my design to have pursued the subject with the hope of discovering the cause, to which so regular a relation might be ascribed.

This one case of a more general law observed by Mr. Dalton.

But since the publication of Mr. Dalton's theory of chemical combination, as explained and illustrated by Dr. Thomson[‡], the inquiry which I had designed appears to be superfluous, as all the facts that I had observed are but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or, if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms.

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[*] Philos. Trans. for 1807, p. 95.

[†] See our Journal, p. 19 and 22 of the present vol.

[‡] Thomson's Chemistry, 3d Edition, Vol. III, p. 425.

A few easy experiments tend to verify this.

However, since those who are desirous of ascertaining the justness of this observation by experiment may be deterred by the difficulties, that we meet with in attempting to determine with precision the constitution of gaseous bodies, for the explanation of which Mr. Dalton's theory was first conceived; and since some persons may imagine, that the results of former experiments on such bodies do not accord sufficiently to authorize the adoption of a new hypothesis, it may be worth while to describe a few experiments, each of which may be performed with the utmost facility, and each of which affords the most direct proof of the proportional redundance or deficiency of acid in the several salts employed.

Subcarbonate of Potash.

Subcarbonate of potash.

Exp. 1. Subcarbonate of potash recently prepared, is one instance of an alkali having one half the quantity of acid necessary for its saturation, as may thus be satisfactorily proved.

Let two grains of fully saturated and well crystallized carbonate of potash be wrapped in a piece of thin paper, and passed up into an inverted tube filled with mercury, and let the gas be extricated from it by a sufficient quantity of muriatic acid, so that the space it occupies may be marked upon the tube.

Next, let four grains of the same carbonate be exposed for a short time to a red heat; and it will be found to have parted with exactly half its gas; for the gas extricated from it in the same apparatus will be found to occupy exactly the same space, as the quantity before obtained from two grains of fully saturated carbonate.

Subcarbonate of Soda.

Subcarbonate of Soda.

Exp. 2. A similar experiment may be made with a saturated carbonate of soda, and with the same result; for this also becomes a true semicarbonate by being exposed for a short time to a red heat.

Supersulphate of Potash.

Supersulphate of potash.

By an experiment equally simple, supersulphate of potash may be shown to contain exactly twice as much acid as is necessary for the mere saturation of the alkali present.

Exp. 3. Let twenty grains of carbonate of potash (which would be more than neutralized by ten grains of sulphuric acid) be mixed with about twenty-five grains of that acid in a covered crucible of platina, or in a glass tube three quarters of an inch diameter, and five or six inches long.

By heating this mixture till it ceases to boil, and begins to appear slightly red hot, a part of the redundant acid will be expelled, and there will remain a determinate quantity forming supersulphate of potash, which when dissolved in water will be very nearly neutralized by an addition of twenty grains more of the same carbonate of potash; but it is generally found very slightly acid, in consequence of the small quantity of sulphuric acid which remains in the vessel in a gaseous state at a red heat.

In the preceding experiments, the acids are made to assume a determinate proportion to their base, by heat which cannot destroy them. In those which follow, the proportion which a destructible acid shall assume cannot be regulated by the same means; but the constitution of its compounds, previously formed, may nevertheless be proved with equal facility.

Superoxalate of Potash.

Superoxalate of potash.

Exp. 4. The common superoxalate of potash is a salt that contains alkali sufficient to saturate exactly half of the acid present. Hence, if two equal quantities of salt of sorrel be taken, and if one of them be exposed to a red heat, the alkali which remains will be found exactly to saturate the redundant acid of the other portion.

In addition to the preceding compounds, selected as distinct examples of binacid salts, I have observed one remarkable instance of a more extended and general prevalence of the law under consideration; for when the circumstances are such as to admit the union of a further quantity of oxalic acid with potash, I found a proportion, though different, yet analogous to the former, regularly to occur.

Quadroxalate of Potash.

Quadroxalate of potash.

In attempting to decompose the preceding superoxalate by means of acids, it appeared, that nitric or muriatic acids are capable of taking only half the alkali, and that the salt which crystallizes after solution in either of these acids has accordingly exactly four times as much acid as would saturate the alkali that remains.

Exp. 5. For the purpose of proving, that the constitution of this compound has been rightly ascertained, the salt thus formed should be purified by a second crystallization in distilled water; after which the alkali of thirty grains must be obtained by exposure to a red heat, in order to neutralize the redundant acid contained in ten grains of the same salt. The quantity of unburned salt contains alkali for one part out of four of the acid present, and it requires the alkali of three equal quantities of the same salt to saturate the three remaining parts of acid.

Sulphate of potash partially decomposed by nitric acid.

The limit to the decomposition of superoxalate of potash by the above acids is analogous to that which occurs when sulphate of potash is decomposed by nitric acid; for in this case also, no quantity of this acid can take more than half the potash, and the remaining salt is converted into a definite supersulphate, similar to that obtained by heat in the third experiment.

Many chemical changes influenced by this law.

It is not improbable, that many other changes in chemistry, supposed to be influenced by a general redundance of some one ingredient, may in fact be limited by a new order of affinities taking place at some definite proportion to be expressed by a simple multiple. And though the strong power of crystallizing in oxalic acid renders the modifications of which its combinations are susceptible more distinct than those of other acids, it seems probable, that a similar play of affinities will arise in solutions, when other acids exceed their base in the same proportion.

Potash will not

In order to determine whether oxalic acid is capable of uniting unite with a triple quantity of oxalic acid.

uniting to potash in a proportion intermediate between the double and quadruple quantity of acid, I neutralized forty-eight grains of carbonate of potash with thirty grains of oxalic acid, and added sixty grains more of acid, so that I had two parts of potash of twenty-four grains each, and six *equivalent* quantities of oxalic acid of fifteen grains each, in solution, ready to crystallize together, if disposed to unite, in the proportion of three to one; but the first portion of salt that crystallized was the common binoxalate, or salt of sorrel, and a portion selected from the after crystals (which differed very discernibly in their form) was found to contain the quadruple proportion of acid. Hence it is to be presumed, that if these salts could have been perfectly separated, it would have been found, that the two quantities of potash were equally divided, and combined in one instance with two, and in the other with the remaining four of the six *equivalent* quantities of acid taken.

Proportions of acid and alkali.

To account for this want of disposition to unite in the proportion of three to one by Mr. Dalton's theory, I apprehend he might consider the neutral salt as consisting of

		2 particles potash	with	1 acid,
The binoxalate as	1 and 1, or	2	with	2,
The quadroxalate as	1 and 2, or	2	with	4,

in which cases the ratios which I have observed of the acids to each other in these salts would respectively obtain.

Perhaps the law depends on a geometrical ratio of the element.

But an explanation, which admits the supposition of a double share of potash in the neutral salt, is not altogether satisfactory; and I am further inclined to think, that when our views are sufficiently extended, to enable us to reason with precison concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

Example.

For instance, if we suppose the limit to the approach of particles to be the same in all directions, and hence their virtual extent to be spherical (which is the most simple hypothesis); in this case, when different sorts combine singly there is but one mode of union. If they unite in the proportion of two to one, the two particles will naturally arrange themselves at opposite poles of that to which they unite. If there be three, they might be arranged with regularity, at the angles of an equilateral triangle in a great circle surrounding the single spherule; but in this arrangement, for want of similar matter at the poles of this circle, the equilibrium would be unstable, and would be liable to be deranged by the slightest force of adjacent combinations; but when the number of one set of particles exceeds in the proportion of four to one, then, on the contrary, a stable equilibrium may again take place, if the four particles are situate at the angles of the four equilateral triangles composing a regular tetrahedron.

This merely hypothetical.

But as this geometrical arrangement of the primary elements of matter is altogether conjectural, and must rely for its confirmation or rejection upon future inquiry, I am desirous, that it should not be confounded with the results of the facts and observations related above, which are sufficiently distinct and satisfactory with respect to the existence of the law of simple multiples. It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known; since even admitting that a very small number of these atoms combining together would have a tendency to arrange themselves in the manner I have imagined; yet, until it is ascertained how small a proportion the primary particles themselves bear to the interval between them, it may be supposed, that surrounding combinations, although themselves analogous, might disturb that arrangement, and in this case, the effect of such interference must also be taken into the account, before any theory of chemical combination can be rendered complete.

III.

Account of Experiments on Sweeping Chimneys, by Mr. GEORGE SMART[*].

Premiums for sweeping chimneys.

THE miseries, to which children employed within the flues in cleansing chimnies are liable, induced the Society for the Encouragement of Arts, &c., to offer premiums in the year 1808, to obviate the necessity of so cruel a practice. In the year 1806, the gold medal was adjudged to Mr. George Smart, of the Ordinance Wharf, Westminster-bridge, for the greatest number of chimneys cleansed under his direction by mechanical means, and a particular account of the method used by him for this purpose will be found in the 23d Volume of the Society's Transactions[†]. During the present session, a gold medal has also been voted to him for the best machine produced to the Society for the intended purpose; an explanatory engraving of which having been given in the volume above-mentioned, renders one unnecessary here[‡]. The following communication has been received from him, and a complete machine is preserved in the Society's repository for public inspection.

SIR,

Experiments. Rope stiffened with whalebone.

Since the middle of February last, I have been trying experiments in chimney-sweeping; my first was, stiffening a rope with whalebone, but found it would not be portable, and that it would be otherwise inconvenient, as in passing from one room to another in the same house, even with the greatest care, it would be almost impossible to avoid touching the paper on the staircases, particularly where they are narrow. In passing through the street, with such a machine, it would be also very troublesome.

If the brush is made to fill the flue, which ought to be the case, a substance of an elastic nature has not power sufficient when the brush is forty or fifty feet up, especially where there are sharp turns in the flues; the force applied to send the brush up is principally spent in friction on the sides of the flues, and of course would soon cut through any flexible substance that combines the whalebone, or other elastic substance used.

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[*] Transac. of the Society of Arts, vol. XXV, p. 97.

[†] See also our Journal, vol. VI, p. 255.

[‡] See Journal, vol. VI, plate 13.

Elastic rods fastened with screws in the joints.

My next attempt was to join elastic rods together by screws in the joints, but this plan would not do in passing sharp elbows, as the joints would be strained, and soon unfit for use, and a danger of the joints slipping or breaking, which would leave the brush in the flue.

Machine formerly described.

I then thought of the simple portable machine I have sent for the inspection of the Society; its cheapness, durability, and power of execution, will, I hope, recommend it. I think with perseverance it will abolish the practice of climbing boys; I have used it in several lofty chimnies, and am convinced it may in time become general. I have also sent a rod and curtain that may be fixed to any opening of a chimney piece, from six inches to five feet, without using nails or forks in the common mode, to the injury of the wainscot or chimney piece.

Method of using it.

My method of working the machine is, by first putting up the brush, then pressing forward one tube after another, as strung upon the rope, till the brush meets with an elbow in the flue; then it is necessary to tighten the rope by pulling it under the feet, or by means of a small pulley, and putting in one of the small screws to pinch the rope; then make a fresh push, and by shifting the two screws, the one to relieve the other, it will pass the elbow, and possess sufficient stiffness to allow the brush to be forced forward to any height.

Brushes.

I have tried the heath and hair brushes, and find, that if the flue is well filled, it does not require so hard a substance as heath, as it brings down the mortar with the soot. The brushes of hair, and those formed from the article of which carpet brooms or whisks are made, I think will answer the best for general use.

This

Tried with success.

This is to certify, that Mr. George Smart, of Cambden Town, by means of a machine of his own invention for sweeping chimnies, has made two experiments on my hall and parlour chimnies, to ascertain the practicability of raising the machine through their various windings. The first of these flues measures upwards of fifty feet from the hearth, and the operation was performed with apparent ease, sending down a quantity of soot, together with some wet mortar, although the flue had been recently swept by a regular chimney ſweeper. The other from the hearth measures sixty feet, and although there are no less than three elbows in it, running in opposite directions, (as the *boy* informs me), the operation was performed within nine minutes.

JOHN TROTTER.

Soho Square, May 2, 1803.

Certificates were also received from Mr. H. W. Dietrichsen, of Pratt place, Camden Town; Mr. Charles Mill, No. 4, Gloucester place, Camden Town; Mr. John Mason, and the Rev. Jeremiah Joyce, of Camden Town, testifying that they had seen Mr. Smart's machine at work, and that they approved thereof.

SIR,

Much used.

I have the pleasure to inform you, that my machine for sweeping chimnies succeeds far beyond my expectation, and that I am not able to attend to one half of the orders I could have for its use.

His Royal Highness the Prince of Wales has directed, that the chimnies at Carlton-house, also those at the Pavillion, shall for the future be cleansed by my machines. I have also had orders to send to different parts of the kingdom my machines ready made, where they have been the means of providing a comfortable subsistance for poor persons not capable of other business.

Price.

The price of a machine to ascend 60 feet, including rod, curtain, extra brush, and box, is £4 14s. 6d.

Advantages.

There are two particular advantages attending my machines; namely, that of sweeping a great number of narrow flues which no child can get up, and that of extinguishing chimnies when on fire, by placing a wet cloth over the brush, and forcing it up the chimney.

The construction of the machine is so fully shown by the description and engraving of it in page 256, of the 23d volume of the Society's Transactions*, that it will be unnecessary to say more upon the subject.

I am, Sir, your humble servant,

GEORGE SMART.

When I first mentioned this machine in the Journal, vol. VI, I promised to see it tried in my own house. This I have since done, and find no reason to modify any thing there said, as it performed its office to my perfect satisfaction.

IV.

Description of a Machine for Cleansing Chimneys, without the use of climbing Boys. By Mr. JOSEPH DAVIS, *No.* 14, *Crescent, Kingsland Road*†.

SIR,

Machines for cleansing chimneys.

I Had the pleasure of submitting to your inspection, a model of a machine for the purpose of cleansing chimneys, on the 3d of May, 1803, and which I wished to be brought before the Society of Arts, being convinced, that the approbation of so respectable and enlightened a body of men would greatly tend towards the superseding the use of climbing boys; and I shall, therefore, feel myself greatly obliged if they would examine the machine, and favour me with their opinion on it.

I am, Sir, very respectfully yours,

JOSEPH DAVIS.

SIR,

The brush part of the model of my machine for cleansing chimnies, which I sent you on the 3d of May 1803, not having

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* Or vol. VI of our Journal.

† Transact. of the Society of Arts, vol. XXV, p. 101. The Society voted the silver medal to Mr. Davis, his machine being conceived next in merit to Mr. Smart's. See the preceding Article.

ing any hair in it, I am now enabled to forward you one in a more perfect state, which I had the honour of using in the presence of his Lordship the Bishop of Durham, at the Military Hospital, Westminster, on the 11th of June, 1803, and at the Jennerian Society, in Salisbury square, Fleet street, on the 22d of the same month, and in the same year;—a certificate of which, signed by the gentlemen who were present*, I had the pleasure of sending you. I have also sent three lengths of the rod, (the same as those which were used at the above places), in order to enable the Committee to judge with greater ease of the construction. Permit me nevertheless to observe, that the principle of my rod is so simple and secure, that it may be used in almost every chimney with safety, either by the maid servant, or a boy of twelve years of age; it is calculated both for private and public use, and a stranger to the machine, who used it at the Military Hospital, declared he could sweep any chimney with it, the plan was so good. It will also sweep German and other stove pipes, and flues of almost every description.. If the gentlemen of the Society will do me the honour to take the machine into their consideration, I will wait on them by hearing from you, and explain farther particulars.

Used with success.

Its advantages.

I am, Sir, your obedient servant,

JOSEPH DAVIS.

SIR,

Remarks on the machine.

I am glad that I have had an opportunity of giving my opinion of your machine for cleansing chimnies, which I have done by signing the certificate you brought me on Monday last. I am of opinion, that your machine is capable of sweeping a very great proportion of the chimneys in London and elsewhere; perhaps there are not more than one or two in a hundred, which it cannot be raised in. At present, there are (as is well known to all chimney sweepers) some chimneys so small, that boys cannot climb them, so that on the whole I imagine, that your machine will sweep about the same number of chimneys as are swept by boys, though not exactly the very same flues in every instance.

I hope in time we shall convince the public, that they can have their chimneys swept with as much cleanliness, and as effectually with machines, as they have heretofore had them done; and I am convinced that they may be swept as cleanly and effectually as is commonly done with climbing boys, so that the difference to the families who employ your machine will be, that they have the same comfort of a clean chimney, and are satisfied, that they no longer use a method which is full of horrors, and a disgrace to a civilized country.

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* B. M. Forster, Joseph Leaper, and James Hebdin, Esqrs.

I remain, Sir,
Your obedient servant,
B. M. FORSTER,

Reference to the Engraving of Mr. Davis's Machine for Cleansing Chimneys, Pl. V, fig. 1, 2, 3, 4.

Description of the machine.

Fig. 1 Represents the upper part of the machine; A A A A are four brushes for sweeping the four sides of the chimney, they are hinged to the bottom of a tube about three inches diameter; B B show two of the four springs which expand the machine to chimneys of all sizes. The heads of the brushes are made about six inches long, and five wide; and form portions of cylinders, the hair being left longer at the top than the bottom. The hair at the ends of the brushes being left still longer, namely, three inches and a half, for the purpose of sweeping the corners of the chimney. C represents the brush at the top of the machine proper for cleansing the pot; the machine may be used either with or without it, but it is very useful for cleansing stove pipes, by being used alone; it is secured to the top of the rod by means of a spring and socket, as the rods below mentioned. D D D D, four lines to draw the brushes near together by a cord E, so that the machine may be forced up the chimney with greater facility. F, the string to expand the brushes when the machine is at the top of the flue.

Fig. 2 Shows on a larger scale the top of one of the rods separate. G, the spring attached to it.

Fig. 3 Explains the manner in which the rods are joined together. H being a brass socket fixed at the lower end of

each rod; the spring G, and upper end of the rod fig. 2, are pressed into this socket so far, that a small point I, on the upper end of the spring, rises through a small hole in the brass socket, and retains the lower end in the socket. Each rod is made of hiccory wood, which being tough and flexible, is particularly well calculated for this use, bending and adapting itself to the different turns it meets with.

Fig. 4 Shows the key to unlock the rods; it is six inches long, and made from a piece of one of the rods, with a steel stud K in the middle, rising a quarter of an inch, and a brass plate on one side projecting the thickness of the rod to guide it into the socket, that it may be used without looking at the rods.

Method of using it.

The method of cleansing the chimney is, by first entering the brush part of the machine, with the brushes closed, and one of the rods attached to it, up the chimney; the head of a second rod is then slided as above mentioned into the socket of the first rod, and the brush by it forced higher up, a third rod is then slided into the socket of the second, and this mode continued till a sufficient number of rods are added to raise the brush to the top of the chimney. The string E, extending from the brush to the bottom of the chimney, being then pulled, occasions the four brushes to expand to the width of the flue, and to bring down with them in their return all the soot which adhered to the sides of the flue. As the machine is drawn down, the rods are separated one by one by means of the key, and laid upon the hearth till the brush part is brought down, which is then closed and laid with the rods.

The usual precautions must be taken of placing a curtain before the fire-place, to prevent the soot, while falling, from flying about the room.

JOSEPH DAVIS.

V.

Account of an Invention to secure the Pannels of Doors and Window Shutters from being cut out by House-breakers. By Mr. JOSEPH DAVIS, *No. 14, Crescent, Kingsland Road*[*].

SIR,

Desirable to prevent pannels of doors or shutters from being cut out.

I HAVE for some time considered, that it would be of great benefit to the public, if a plan could be adopted to prevent the pannels of shutters or doors being cut out by house-breakers; and having tried a great number of experiments, I have at length succeeded in accomplishing the one I have the honour of forwarding to the Society for the Encouragement of Arts, &c.

Method of effecting this.

This improvement consists in introducing tempered steel wires through the pannels and stiles at the distance of three inches, thereby not only making a door or shutter far superior in strength, but calculated to defy the attempts of the house-breaker in taking out a pannel. This improvement, though so far superior to any hitherto known for appearance and utility, will be attended with less expense. I have submitted the above plan to Messrs. Paynters, Coleman street, likewise to Messrs. Moffat and Co. Paternoster row, and to several gentlemen in that line of business, who all do me the honour to say, that it far surpasses any thing of the kind, that they have ever known or conceived, and that it will completely answer the purpose.

I have also sent the machine on which I bored the pannel, conceiving it to be an additional recommendatoin; with this machine, a boy may with ease bore the shutters, &c. which otherwise might be difficult for a man to accomplish.—I have the pleasure most respectfully to subscribe myself,

SIR,

Your very humble servant,

JOSEPH DAVIS.

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[*] Transactions of the Society of Arts, vol. XXV, p. 101. Ten Guineas were voted to Mr. Davis for this invention.

CERTIFICATE.—We hereby certify, that Mr. Davis's improvement on doors and shutters, to prevent the pannels from being cut out by house-breakers, is the best that we have ever seen; and we are of opinion, that its being known will be an advantage to the public, and do therefore recommend it to the Society for the Encouragement of Arts, Manufactures, and Commerce, for their consideration.

F. PAYNTER and Co. Coleman street.
E. COLEBACH, Minories.
J. TEASDALE, Paternoster row.
W. ROLFE.

April 22, 1807.

Reference to the Engraving of Joseph Davis's Invention for securing Window and Door Pannels, Pl. V, fig. 5, 6, 7.

Explanation of the plate.

Fig. 5. Represents a wooden pannel made in the common way, the dotted lines show the situations of the tempered steel rods within the pannel, the holes through which the rods were introduced on one side being closed up.

Fig. 6. Shows a section of the same, the small dots in the engraving denoting the place of the rods.

Fig. 7. Shows the instrument on which the pannels are laid to be bored. The borer passes through the holes L M of the two upright pieces, which keep the borer in a straight line to act upon the pannel laid upon the frame N.

VI.

Description of a new Watch Escapement. By Mr. S. MENDHAM, *Counter Street, Borough*[*].

SIR,

Principle of a new escapement.

I Beg leave to lay before the Society a model of a new escapement, the principle of which is, that the balance acts without friction, and the movement in itself very simple;

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[*] Trans: of the Society of Arts, vol. XXV, p. 108. The silver medal of the society was voted to Mr. Mendham for this escapement.

the impulse is given without jarring, the inequality of power through the train has no perceptible effect on the balance; and no additional weight, however great, can produce more than a regular and gentle increase of impetus on the balance.

I remain, Sir,
Your most obedient servant,
S. MENDHAM.

SIR,

Opinion respecting it.

Having attended the Committee upon Mr. Mendham's escapement, I think it justice due to a man of genius to give my opinion farther upon it.

In viewing mechanical improvements, we should not confine our ideas to their present properties, but should consider what improvements the principle will admit of.

Compared with Mudge's.

As the principles of Mr. Mendham's escapement, and that of Mr. Mudge's, which obtained a bounty from government, are much the same, I shall compare the one with the other.

The impulse given to the balance without friction is exactly the same as Mudge's. The remontoir is bent up by the maintaining power in a similar way to that of Mudge's, but from the form of the pallet, which is a plain surface, it is not so perfect. Mudge's, from the form of the pallet, bends the remontoir always to the same place, the other is bent higher or lower according to the force of the maintaining power, but by forming the pallet like Mudge's it would render them alike in this respect. The only other objection is the spring detent, that detains the wheel, when it drops from the pallet of the remontoir; it is the same as that of a detached escapement, consequently exposed to the whole force of the maintaining power. To compensate for these objections, the arc of vibration is not limited like Mudge's, which is of great importance, and, having only one remontoir, it is more simple. It is, therefore, superior to Mudge's in having only one remontoir, and being unlimited in the arc of vibration; it is superior to the detached escapement in giving the impulse without friction.

I am, Sir,
Your very humble servant,
THOMAS RAMSAY.

Reference to the Engraving of Mr. S. Mendham's Escapement, Pl. V. fig. 8, 9, 10.

Properties of the invention.

In the escapement referred to, there are two principal peculiar properties in the invention, both which I consider superior to any thing of the kind laid before the public; first, the balance is kept in motion without any friction whatever, and in a manner so simple, that even movements of inferior workmanship must go with great accuracy.

Being not in this line of business, or acquainted with any persons in the trade, where I might have had an opportunity of examining different escapements, I certainly labour under many disadvantages; for since I have been honoured with the Society's medal, I have heard of an escapement by which the balance is kept in motion without friction, but being limited in the arc of vibration, complicated, and very expensive in the movement, it renders it much inferior to mine.

The impulse not given by a stroke.

In the second place, the balance is kept in action by an impelling power without any blow whatever; all other escapements, which have fallen within my notice, have kept up the vibration by a direct blow virtually on the balance itself, which I have always considered to be a great disadvantage; for a blow upon any thing of the nature of a spring produces that kind of shock, which can by no means be convenient or serviceable in keeping a steady motion, which is so essentially necessary, but is on the contrary disadvantageous.

Explanation of the plate.

The figure, Plate V, fig. 8, represents the escapement without the rest of the train; *a a* are the two plates of the frame between which the train runs; *b* is the last, or balance wheel of it, with teeth nearly similar to that of the balance wheel of an eight-day clock, moving with the flat face of the tooth forward against the pallet *c* of an upright spindle *d*; *e* is a locking spring nearly similar to a detached one, having no extra spring to pass to and fro with. Above the pallet *c*, is a very small one, *f*, which is for the purpose of unlocking the wheel, which is better shown in fig. 9; at the lower part of the spindle *d*, is a hair spring *g*, so pinned as to bear the pallet *f* against the locking spring with sufficient power, so that of its own accord it frees the wheel and lays the pin *h* which comes through the plate gently up to the stop, consequently the tooth falls upon the pallet *c*, but so close home to the centre of the spindle, that it has no power to pass it of its own accord; the pin *h* referred to is fixed to the top extremity of the pallet *c*, and rises perpendicularly through the plate *a* some way above the surface.

The balance *i* is fixed on the centre of its spindle, principally on account of equalizing the weight, beside which it is the most convenient to be so; it is supported between the plate *a*, and the cock *k*, precisely over the spindle *d*, consequently the action of each is in the same arch, and the connection is between the pin *h* of the pallet, and the pin *l* of the balance, (a pin fixed in the balance at the same distance from its centre as the pin *h* is from the centre of the spindle *d*, and sufficiently long to touch the pin *h* sideways), there is therefore no friction whatever between them.

Mode of its action.

Having mentioned the different parts of the escapement, I shall proceed to explain its action. The immediate course of vibration is from the spring *g*. The balance spring is so placed, that the pin *l* of the balance stands near the pin *h* of the pallet. It is to be remembered, that the tooth of the wheel rests on the pallet during the vibration of the balance, so that, when the balance is put in motion, the pin *l* comes in contact with the pin *h*, which stands perpendicularly almost imperceptibly fine, and carries it back; as soon as moved, the tooth of the wheel gives it an extra assistance of about one fifth of a circle, passes and lays the next tooth on the lock; on the return of the balance, the spring *g* applies all its power in urging the balance forward till it comes to the stop, the balance then maintains its motion, and the small pallet *f* having unlocked the wheel, the tooth falls again on the great pallet *c*, and waits the return of the balance.

The balance carrying the piece *h* back forms a very admirable banking without any extra apparatus, which is generally done by some kind of stop on the hair spring, which must have an irregular effect; the farther the pin is carried back, the stronger the spring operates against it, and from the extent where the piece may be forced back to, there is play for near two whole circles of vibration, without any possibility of upsetting. The balance of the model vibrates about a circle and one third with extraordinary freedom, *though* a course train of four wheels, a large and heavy balance, with only the power of a stout watch spring. I therefore think the power necessary to carry a train with this escapement may be considerably less than any other of a detached nature.

Fig. 9 represents the axis *d* shown separately, in order that the arm and pin *h*, and little pallet *f*, may be seen more distinctly.

Fig. 10 shows the balance wheel *b*, and the method of locking and unlocking.

S. MENDHAM.

VII.

On the Fecula of Potatoes, and some other British Vegetables. By Mr. WILLIAM SKRIMSHIRE, *Jun.*

To Mr. NICHOLSON.

SIR, *Wisbech, Oct.* 11, 1808.

I Take the liberty of sending for insertion in your valuable Journal my promised communication on the fecula of potatoes, and some other vegetables growing in my neighbourhood.

I remain, yours, &c.

WM. SKRIMSHIRE, JUN.

Assertions in a former paper verified by far-

Since the paper published in the ninety-first number of the Journal was written, I have verified by the following experiments

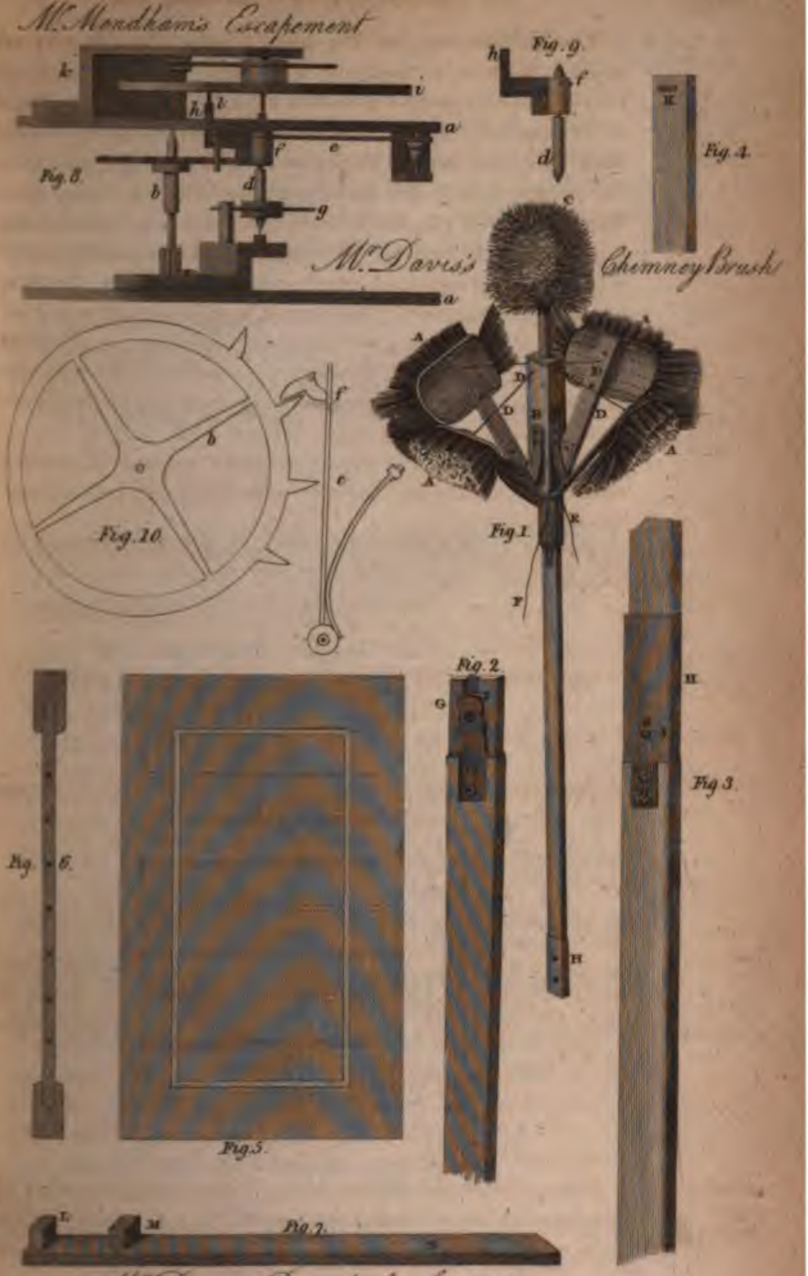
Mr. Mendham's Escapement
k
h
l
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Fig. 8.
b
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Fig. 9.
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Fig. 4.
H
Mr. Davis's Chimney Brush
A
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Fig. 1.
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Fig. 10.
Fig. 2.
G
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Fig. 3.
Fig. 6.
Fig. 5.
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Fig. 7.
Mr. Davis's Pannels for Security

periments two assertions, which were there made, viz. that the quantity of fecula procured from the solanum tuberosum is influenced by the mode of operating, and by the precise state of dryness of the fecula at the time it is weighed.

ther experiments.

Hundred eyes potato.

In August last, 1000 grains of the young roots of the potato called hundred eyes, which had not arrived at their full growth, were grated, and, by the same manipulation as was employed in the former experiments, afforded

	Grains.
Fine white dry fecula	99
Discoloured fecula	6
Dry pulp	71
Water, soluble mucilage, and extractive matter	817
	1000

The same quantity of these roots being grated and repeatedly triturated in a mortar, with frequent edulcoration, and pressing the pulp with the hands, afforded

Fine white dry fecula	111
Discoloured fecula	20
Dry pulp	44
Water, soluble mucilage, and extractive matter	825
	1000

Difference of ·026 of fecula from greater care.

Thus by greater care and attention bestowed in the last process, we gained twenty-six grains more fecula from one thousand grains of the fresh root, than we procured by the first method of operating.

127 grs apparently dry lost 16 by heating,

When the fine white fecula procured in the last experiment was at first separated, it was placed in a window facing the south for two days, exposed to the air, and frequent sunshine, until it felt and appeared perfectly dry, it then weighed 127 grains; but being farther dried upon an iron plate, with a gentle heat for two hours, and put into the balance while it was sensibly warm, it weighed 111 grains.

and then absorbed 29 from the air of a cellar.

It therefore lost 16 grains in weight after it appeared to be quite dry. It was afterwards placed in a cellar for twelve hours, and although it yet appeared dry, it now weighed 130 grains.

Fecula of some potatoes absorb less.

Hence we learn, that dry fecula will absorb more than one sixth of its weight from the atmosphere. But it is more than probable, that this property may vary in the different species of fecula; for some which was obtained from an early potato, subject to a similar experiment at the same time, did not absorb quite one seventh of its weight from the atmosphere.

Fecula still left in the fibrous part.

So far is the common process, that is employed for procuring the fecula of the potato, from separating the whole of this principle from the fibrous part of the root, that when the pulp thus obtained is dried in the air, or by heat artificially applied, an ounce of it boiled for a few minutes in twenty-four ounces of water will gelatinize that quantity, which being sweetened with sugar, and flavoured with a little wine and spice, very much resembles sago that is thus cooked. Indeed it is in my opinion equally palatable and nutritious with that more costly article of food: for which it may be economically substituted in every case, and with every advantage that can be derived from the use of sago.

The pulp should not be thrown away.

For this reason, whenever potato fecula is procured according to the method formerly described, it will be highly improvident and wasteful, to throw the pulp away as refuse, or even to feed pigs with it in its crude state, as has been recommended by some authors; since by being boiled for a very few minutes only, in a large quantity of water, it is converted into the most nutritious food, that any animal can be fed with, and I have no doubt but it will fatten them as effectually, and expeditiously, as any other food that is usually employed for this purpose.

Substitute for coffee.

If the pulp when first separated, and before it is dried, were formed into thin cakes, and roasted with a small quantity of oil or butter, in an iron pan, until it is quite brown and dry, I think it may be used as coffee, and prove an excellent substitute for that costly berry. At least it will prove far superior to that execrable trash, which is often vended under the title of English coffee.

I have frequently formed a very grateful and nutritious beverage from potatoes sliced, roasted to a coffee colour, then ground in a mill, and mixed with a sixteenth part of its weight of the best Turkey coffee.

Useful when boiled for fattening pigs, & perhaps other animals.

That the preparation of vegetable fecula with boiling water will prove the most economical method of fattening pigs, I have very little doubt. And perhaps it will be found equally useful when employed for fattening the ruminant animals. However, this is an object worth the attention of graziers, and others concerned in feeding animals for the market.

Method of preparing it.

The mode of preparing the food, which I recommend, is as follows. Let the proper quantity of potatoes be ground, or rather grated in a mill formed for that purpose, in a large quantity of cold water, which should remain at rest for some hours, and then be decanted off. The whole sediment, both pulp and fecula, should be mixed with a proper quantity of water, and boiled in an iron boiler, frequently stirring it before it begins to boil. When it has boiled a few minutes, and is cooled down to a proper temperature to be eaten, it is fit for use.

May be mixed with other food.

Should prejudice prove so inveterate as not to trust to potato gruel alone, no one can object to using it as an auxiliary, mixed with barley-meal, boiled potatoes, or pounded linseed cakes.

Nutriment increased by boiling.

The increase of nutriment by boiling farinaceous vegetables in water is so great, that I cannot refrain from recommending its general use; being confident, that the advantage gained by it will amply compensate for the labour and expense attending the operation.

Recommended as good for man.

But it is not as a food for quadrupeds alone, that I wish to facilitate the introduction of potato fecula. I can safely recommend it as a very palatable and nutritious food for man. And however the generality of my countrymen may be disposed to ridicule the notion of being fed upon potato gruel, I fearlessly recommend the following useful and economical preparation of this inestimable root.

Useful & economical preparation from potatoes.

The potatoes being grated, or rasped, in a large quantity of water, and allowed to stand some hours, the water may then be decanted off, and more added, which, after standing an hour or two, may likewise be poured away, and still more added if necessary, until it no longer acquires any taste or colour. The whole fecula and pulp are then to be well mixed together, formed into small cakes, and dried in the air, or by a gentle heat. This is a preparation, which will keep for many years—which no family ought to be without, and which is in the power of the poorest family to possess.

Jelly from it, soup, & other preparations.

Half an ounce of this preparation will gelatinize so large a quantity of boiling water, as to afford a sufficient meal for any labouring person in health. It may be sweetened either with molasses or sugar: or being boiled with an onion and pot herbs, and seasoned with pepper and salt, it will make a very palatable, wholesome, and nutritious soup. But should the raw flavour of the potato predominate, as will sometimes happen, when the preparation is newly made, it may be corrected, and the soup improved, by the addition of a little mushroom catsup, allspice, anchovy, or red herring.

Useful for the sick, convalescent, enfeebled, or children.

If this preparation of the potato be boiled with milk, sweetened with sugar, and flavoured with a little wine or spice, it forms the most nourishing and restorative food, that can possibly be administered to the sick and convalescent.

It is so easily digested, that it soon becomes animalized, even by the impaired functions of a debilitated constitution. Thus it is peculiarly fitted to the digestive organs of the debauchée, and to the languid powers of infancy. And I have known infants wholly nourished for months by this preparation, boiled in milk and water, sweetened with a little sugar.

Excellent remedy for sore throat.

With a larger proportion of the preparation a stiff jelly may be formed, which acidulated with lemon juice, or any other vegetable acid, becomes the best domestic remedy that can be employed, in every species of sore throat.

More elegant preparation.

Those who can afford it may have a much more elegant, though rather a more expensive article, in the pure fecula itself, deprived of all the pulp and fibrous part of the potato. This preparation is so easily made, that I hope to see it introduced into general use. And I do not hesitate to say, it will be found superior in every respect to salep, sago, arrow root, or any of the vegetable preparations of that kind, which have been so pompously advertised, and recommended to the public, by those who are interested in the sale of them.

Potato starch.

Indeed it is already generally known to laundresses under the name of *potato-starch*, and they are no strangers to the method of procuring it from the fresh root; but they are not sufficiently aware of the nutritious property which this substance possesses. And it is principally with the view of making it more generally known, that I am induced to lay before the public these experiments and observations*.

The science of nutrition little known.

It will appear ludicrous to many, to assert in the present age of the world, that the science of nutrition is yet in its infancy; but truth obliges us to confess, that such is absolutely the fact. The cause of our ignorance it is not my intention to investigate.

Cookery.

It is, I believe, a general opinion, that the nutriment of our food, especially the vegetable part of it, is greatly increased by cooking. This is therefore an art, which claims the attention of the whole human race. It is an art, so intimately connected with the welfare of our species, that it is absolutely essential to its existence, in a state of civilized society.

Water essential to nutrition.

In the present tottering state of the Lavoisierian doctrine of chemical science, it is fortunately of no consequence to our subject, whether water be a compound substance or a simple element. And we have no fear of contradiction when we assert, that it is essential to the nutrition of animals, as well as of vegetables.

In its simple state assists digestion mechanically

When water in its simple state is taken into the stomach along with our food, its principal effect in assisting digestion is perhaps mechanical only, by giving the food a *certain* degree of consistence, most favourable for the gastric fluid to act upon it, and according to Mr. Home's late important discovery, the superfluous quantity is conveyed into the circulation by the intervention of the spleen*. For as the whole internal surface of the stomach is endowed with the power of producing a secretion, which possesses the property of coagulating albuminous fluids, before the organ is enabled to convert them into chyle, we have some show of reason for supposing a *certain* degree of consistence in the contents of the stomach to be most favourable, if not absolutely necessary, to the healthy action of the digestive organs.

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* Many years ago an article was sold in canisters by the name of sago powder, which I believe was chiefly if not solely made from potatoes: but it fell into disuse, whether from prejudice alone, or from negligence in preparing it, I cannot say. I also remember the fecula of potatoes being strongly recommended as a substitute for salep, particularly as keeping better, as well as for sago, by a writer in the Journal de Médecine. C.

* See Journal, p. 103 of the present vol., and p. 347 of vol. XX.

But when chemically combined with fecula becomes animalized.

But when water is gelatinized by its chemical combination with fecula and heat, there is considerable reason to believe, that even the whole of its particles become animalized by the efforts of the stomach. The importance of this idea is such, as to require it to be impressed upon the minds of all, who wish to study the science of nutrition.

Those substances most useful that gelatinize most water.

If therefore the digestive organs have the power of animalizing water, after it has acquired a *certain* degree of consistence, by boiling with farinaceous vegetables, or other substances, we may conclude, that these preparations of vegetables, which in the process of cooking are enabled to consolidate or rather to gelatinize the greatest quantity of water, will be found to afford the largest portion of nutriment, and are consequently the most beneficial to mankind.

Potato fecula recommended.

It is from these considerations, that I am induced to recommend in the strongest terms of approbation the use of potato fecula, as being by far the most economical method of employing this inestimable root.

Common starch not to be taken as food.

From what has been said above, concerning the nutriment of potato starch, I do not wish it to be understood, that the common starch of the shops may be administered as food with impunity. For common starch, after having undergone a slight fermentation, which is sometimes produced by the addition of impure and nauseous ingredients, is still farther contaminated by a metallic oxide, which is probably inimical to the human constitution.

It is for this reason, I wish to retain the name of *fecula*, instead of *starch*, as a generic term for this vegetable principle.

Fecula from various vegetables.

Reflecting upon the nature of this vegetable product, and considering it is equally the produce of seeds and roots, and that, if it be procured with care, it is perhaps equally nutritious from whatever plant it is obtained; whether it be in the form of fecula from the wheat of Great Britain, or in that of Cassava from that deadly poison the Jatropha Manihot of North America; I was induced to suspect, that some other British vegetables, both indigenous and naturalized, might be rendered much more serviceable to our species, than they are at present supposed capable of becoming.

With this view I selected the following.

1. *Æsculus Hippocastanum, or Horse Chesnut.*

Horse chesnut.

Of the fruit of this tree, fresh-gathered, peeled, and skinned,

1000 grains, rasped in water with a coarse file, afford

	Grains.
Fine white dry fecula	200
Discoloured or yellowish fecula	32
Dry pulp	80
Water, soluble mucilage, oil, and extractive matter	688
	1000

Thus we find the fruit of the horse chesnut contains more than one fifth of its weight of fecula, the whole of which is converted into animal matter, in the process of digestion! We may therefore, in a time of scarcity, accept with gratitude another rich and wholesome fruit, which has hitherto been held in little estimation.

And indeed at a time when there is no scarcity, those persons, about whose habitations this handsome tree is found to flourish, may profitably employ its fruit in the manner here pointed out.

2. *Quercus Robur, Common Oak.*

Acorns.

The acorn affords a considerable quantity of fecula, but its colour, which is a dirty light brown, similar to powdered salep, will always detract from its value, and prevent its introduction to general use, so long as a more elegant article can be procured with equal facility, and at the same expense. However, I am fully persuaded its colour does not injure its nutritive property.

1000

1000 grains of this fruit fresh gathered, and not quite ripe, when peeled and skinned, afford

	Grains
Dry brown fecula	165
Dry pulp	150
Water, soluble mucilage, oil, and extractive matter	685
	1000

3. *Bryonia dioica, Red-berried Bryony, or as it is vulgarly termed, Mandrake.*

Root of Red-berried bryony, or mandrake.

This plant, which is common in this neighbourhood, has a very large, thick, white root, and although it is one of the most violent drastic cathartics, which this kingdom produces, it may, by a similar process to what we have before described, be made to afford a very fine white nutritious fecula, in great abundance.

1000 grains of the fresh root dug up early in May, afford

	Grains.
Dry white fecula	50
Discoloured fecula	45
Dry pulp	50
Water, soluble mucilage, and extractive matter	855
	1000

4. *Arum maculatum, Cuckow-pint, or Wake Robin.*

Root of arum, cuckow pint, or wake robin.

The root of this plant, which is very plentiful in my neighbourhood, although one of the most acrimonious vegetables of British growth, may, by particular management, be converted into a very rich, wholesome, palatable, and productive food.

It is excellent, eaten either boiled or roasted, particularly by the latter mode of cooking. If formed into vermicelli, it is a beautiful preparation. When dried it may be made into bread; and when treated according to the method above mentioned for procuring fecula, a much greater quantity may be obtained, than from any vegetable hitherto operated upon*.

1000 grains of the fresh root, dug up early in May, afford

	Grains.
Very pure white dry fecula	254
Dry pulp	28
Water, soluble mucilage, and extractive matter	718
	1000

Fecula very generally diffused.

The more we reflect upon the general diffusion of this nutritious principle throughout the vegetable kingdom, the greater occasion have we to be seriously and unfeignedly thankful to that Almighty Being, whose extensive benevolence has thus bountifully placed within the reach of man a sufficiency of nutriment, in every corner of the Earth!

VIII.

Remarks on Meïonite, with some Observations on a Paper by Mr. FREDERIC MOHS, *in which this Substance is considered as a Variety of Feldspar. By Mr.* TONNELIER, *Keeper of the Cabinet of Mineralogy to the Council of Mines*†.

Is meïonite a species, or a variety?

DOES the mineral mentioned by the name of meïonite in the *Tableau méthodique* of Mr. Haüy constitute a distinct species, or is it merely a variety of some species formerly known? Such is the question, that suggested itself to me, on reading a paper by Mr. Frederic Mohs, lately inserted

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[*] The root of this plant has been employed for making starch in the island of Portland, from time immemorial. Some years ago the Society of Arts gave a premium to a person of that island for an account of the process, with a specimen of the starch.

[†] Journal des Mines, vol. XX, p. 165.

in the Ephemerides of baron Moll*, and which I shall here attempt to answer.

First noticed by Romé de l'Isle as a jacinth with some other substances.

For our first knowledge of this substance we are indebted to Romé de l'Isle. This philosopher, guided by the analogy of the figures of their crystals, has united under the name of jacinth, in the second edition of his immortal work on crystallography, several substances, that now form distinct species. These however he was far from considering as the same, though he did not think proper to distinguish them by different names, which he must have invented for the purpose. In the description he has given of the second variety of the jacinth the dioctaedral variety of the meionite is easily recognized. Beside its locality in the lava of Somma, and the white colour of the mass, which are pointed out, it is there said, that the two quadrangular pyramids of the jacinth, the primitive zircon of Haüy, are separated by a prism of eight unequal faces, alternately hexagons, and rectangular parallelograms; and that the latter of these, produced by the truncation of the edges of the prism, sometimes very narrow and scarcely perceptible, and at other times more or less broad, always answer to the faces of the pyramids; while the hexagonal sides of the prism are always intermediate to these faces: circumstances that agree perfectly with the dioctaedral meïonite. See pl. 6. fig. 5.

These separated into four species by Haüy,

Mr. Haüy has made four species of the substances described by Romé de l'Isle.

1. The zircon (the jacinth and jargon of former mineralogists), divisible into an octaedron with isosceles triangular faces, which may be subdivided parallel to planes that would pass through the summits and edges of the faces.

2. The harmotôme (cross-stone, crucite, andreolite of the Hartz, staurolite of Kirwan), divisible into a rectangular octaedron, subdivisible on the edges contiguous to the summit.

3. The idiocrase (vesuvian of Werner, jacinth of volcanoes) divisible parallel to the faces and diagonals of a right prism with square bases, differing little from a cube.

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* Ueber Haüy's Mejonit, von Friedrich Mohs: in the Efemeriden der Berg- und Hüttenkunde, herausgegeben von Carl Ehrenbreit Freiherrn von Moll, Band II, Lieferurg I. Nurnberg, 1806.

4. The meïonite (Romé de l'Isle's white jacinth of Somma), divisible parellel to the faces of a right prism with square bases. Fig. 4.

On the principle generally followed by him,

and the results of which have been generally adopted.

In characterising these species the learned author of the Theory of the Structure of Crystals has merely applied the general principle, that has served as the base of the classification of the species in the system published by him. On this occasion he had followed the same course, as he had pursued when he separated the heterogeneous substances of which the former mineralogists composed the species they termed schoerl, in order to make a proper distribution of them, or when he demonstrated four distinct species to have been confounded together under the name of zeolite. In short, to constitute the species meïonite Mr. Haüy has employed the means, of which he has so successfully availed himself to effect those useful reforms, for which mineralogy is indebted to him, and the result of which has been a more precise definition of species, with a more regular classification of subjects. The title that meïonite has to be admitted into the system as a species therefore is equally incontestible with those of several other species established by the same gentleman, and generally adopted.

What is the meïonite in Werner's system?

Among those who come to study the mineralogical collection of the council of mines, which adds to the means of information derivable from the number and variety of its specimens the advantage of being able to compare the methods of two of our greatest masters, several have put to me the following question: "What species in Werner's system corresponds with that which Haüy has designated by the name of meïonite?" Hitherto I had been unable to answer this question, notwithstanding the pains I had taken to procure the printed or manuscript syllabuses of the mineralogical lectures delivered at Freyberg. On the one hand I could not suppose that the meïonite, which is at present to be found in all public and private collections, should be wanting in that of Mr. Werner; and on the other hand, in the series of families given by that illustrious professor, into which are adopted without any change of name several species established by the celebrated professor of the Museum of Natural History at Paris, there was no mineral that I could take for the substance in question. I remained in this uncertainty, till I had read the paper of Mr. Mohs, in which we are informed, that Mr. Werner had not yet adopted the meïonite of Haüy as a distinct species, suspecting it may be nothing more than a simple variety of feldspar. Now the object of this paper is to demonstrate the reality of what is but a simple conjecture on the part of a naturalist, who knows when to doubt, and when to decide. On reading it with that double attention, which the name of Werner and the talents of Mr. Mohs would naturally inspire, I felt in spite of myself the regret of not being able to embrace the same opinion respecting the nature of the mineral that forms the subject of the present paper.

He suspects it to be only a variety of feldspar,

They do not agree in figure;

Mr. Mohs admits, that the characters taken from figure exhibit great differences between the meïonite and the feldspar; and he confesses, that these differences are little capable of being reconciled: but, as he thinks it not altogether impossible, to reduce the forms of the meïonite to a very simple form, which he has observed in the series of forms presented by feldspar, he has flattered himself with being able to justify completely the suspicion of Mr. Werner. The geometrical form in which he gives the proofs alleged in his paper has enabled me to combat them with the same weapons. I appeal therefore to these geometrical reasons, which are employed with the more propriety in the present case, because it is only by the help of the nicest precision, that an able hand has traced the line of demarcation between the meïonite and other species of the mineral kingdom.

but supposed to be reducible to one common form.

A species can have but one primitive form, and its integrant molecule the same.

It is a principle generally admitted, even by the confession of Mr. Mohs*, that in a mineral species there can be but one primitive form, and one single form for its integrant molecules. To prove therefore, that the meïonite cannot be a variety of feldspar, it is sufficient to demonstrate, that the primitive forms and integrant molecules of these two minerals are very different.

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* "Es ist ein grundsatz dass in einer gattung nur eine kerngestalt, und nur eine integrirendes molecul, vorkommen koennen; und der orictognost....traegt kein bedenken diesen grundsatz in seiner vollen allgemeinheit gelten zu lassen." Ephemerides of Baron Moll, Vol. II, Part I, p. 15, 1806.

I. Feld-

I. *Feldspar.*

Primitive form of feldspar.

The primitive form of the feldspar, according to Haüy*, is an oblique angled parallelopipedon, in which the angle of incidence between M and P is of 90°, that between M and T of 120°, and that between T and P of 111° 28′ 17″ See in fig. 1 this solid represented in the position given it by Mr. Mohs himself, as being favourable to the comparison he makes of the two substances. Mr. Haüy observes in his treatise, it is true, that the sections parallel to M and P are very clear, and very easy to obtain; while that parallel to T simply shows itself by a changeableness of colour in a strong light. Since the publication of this treatise however, this gentleman has obtained from the feldspar, by mechanical division, nuclei presenting the joint parallel to T in a very clear and decided manner, which he has publicly shown in his late courses of lectures, and some of which he has distributed among his auditors.

Can the forms of the meïonite be produced by any decrement of this? No.

The primitive form of the Feldspar once thoroughly ascertained, it remains to be known, whether, setting out from this nucleus, we can obtain by the laws of decrement the forms of the meïonite. But the mere inspection of the crystals shows at once the impossibility of this. In fact, the meïonite has the four faces of its summit equally inclined to each other and to the lateral faces. Now this symmetry is incompatible with any primitive form but a prism with square bases, as in the mesotype, or a rectangular octaedron, as in the zircon; both which species exhibit forms analogous to those of meïonite, but with different incidences. It is altogether the reverse with the forms of the feldspar, which bear in some sort the impression of the irregularity of their primitive form in the want of symmetry of the faces arising on parts similarly situate. The following details appeared to me necessary, to place this proof in a clearer and stronger light.

Two crystals most favourable to the supposition taken.

Fig. 3 represents one of the forms of feldspar, in which the faces M, P, T of figure 1 are preserved, and the face O results from the decrement $\frac{F}{2}$ according to the position the

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* Traité de Minéralogie, tom. 2, p. 591.

nucleus has here. Mr. Mohs has chosen this form among all those of the feldspar, as being the simplest, and best calculated to lead to the object he had in view, the reduction of the forms of the meïonite to those of the feldspar. On the other hand fig. 4 represents the dioctaedral meïonite. It now remains, to compare these two forms together; and this, I must apprise the reader, is the essential point of the discussion.

These do not agree.

Mr. Mohs, having measured the angle of incidence between T and P in the crystal of feldspar fig. 3, found, that it agreed manifestly with that between *l* and M in the dioctaedral meïonite, fig. 5. In fact we find by calculation a difference of 21′ only between these angles; the first being 111° 28′, the second 111° 49′. But on proceeding with the comparison, instead of evident resemblances, we have nothing but striking differences. For instance, the angle of incidence between *l* and each of the two sides M, M, is the same; while that between T and M, fig. 3, differs 8° 32′ from that between T and P, since it is of 120°. On the other hand, the angle between O and M is of 116° 21′, and that between O and the face opposite to P is of 124° 15; yet each ought to be of 111° 49′ for the form of the meïonite to agree with that of the feldspar. It is the same with all the other faces, that can arise on the edges or angles of the face T. There are none similarly situate but the faces analogous to M and S, fig. 5, the angles of incidence of which are 90° and 135°. But this is only an accidental resemblance owing to the symmetrical position of the lateral faces in the two nuclei; otherwise we might say, that feldspar is an ore of oxide of tin, since the same angles of incidence are found on the prism of the latter. As to the essential difference between the summits of the crystals of feldspar and those of the crystals of meïonite, this is owing, as has already been observed, to a want of symmetry in the positions of the bases of the nucleus with respect to the lateral faces, which does not allow the faces produced, in consequence of the laws of decrement, to preserve that regularity with respect to each other, which appears in the terminal faces of the dioctaedral meïonite.

The meionite

So far then from acknowledging with Mr. Mohs, that we meet with no crystalline face in the meïonite, the inclination of which does not occur among the forms of feldspar, we will venture to request that gentleman, to endeavour to derive the figure of the dioctaedral meïonite, represented fig. 5, from the primitive form of the feldspar, so that the angles of incidence between all the contiguous faces shall agree exactly; I say, exactly, for in such cases every thing depends on precision; and he will soon convince himself of the impossibility of succeeding. Now this consideration alone is sufficient, to set aside for ever the idea of uniting the meïonite with the feldspar, and decides the question beyond dispute.

not deducible from the primitive form of feldspar.

This decides the question.

The author of the memoir, after having asserted, that all the faces of the meïonite may exist in the feldspar with the same inclinations, finding that the angles of incidence mentioned by Mr. Haüy differ evidently from each other, ascribes this difference to errours in the goniometer, and a want of agreement in the data; and he leaves it to the skilful oryctometer, to remove the difficulty that this want of harmony presents. But the crystallographer finds nothing here to reconcile, since every thing is regular in each of the two crystalline forms. The incidences of the faces have that relation to the primitive forms proper to each species, which calculation, agreeing with observation, indicates in a precise manner by virtue of certain laws of decrement. If the angle of incidence between T and P approach that between *l* and M; if those between O and M and O and P differ from the latter, as well as from each other; it is because the form of the integrant molecules and the laws of decrement require it. These laws have been determined with the more certainty, as there has been no difficulty in procuring well defined crystals of feldspar and meïonite. They who are fully acquainted with the theory of Haüy, and at the same time know the precision, with which he applies it, see no difficulty in the case. They know, that the angles are rigorously determined by calculations founded on certain laws of decrement, the truth of which is in turn confirmed by the agreement of observation with calculation; and they require no more.

Difference ascribed by Mr. Mohs to errours in measuring.

But the form of each is regular, distinct, and agreeable to the theory.

One example will be sufficient, to give an idea of the accuracy

Ins'ance of the

nicety of Mr. Haüy's measurements.

curacy of the measures given in the work of Mr. Haüy. It is in p. 39 of the first volume of his *Traité de Minéralogie.* Among the number of forms exhibited by sulphuret of iron may be observed the dodecaedron with pentagonal faces. This crystal is divisible parallel to the sides of a cube; which is the form of its nucleus, and at the same time that of its integrant molecules, which perform the functions of subtractive molecules. On each face of the primitive cube two simultaneous decrements are supposed to take place in the additional laminæ; one of two rows in breadth, setting out from two opposite edges; and one of two rows in height, setting out from the other two edges of the same face. The decrements that take place on the faces contiguous to the nucleus follow the same laws, and in directions crossing each other, so that the slower decrement on one face answers to the more rapid decrement on that contiguous to it. The nature of the decrements, added to the direction of the laminæ, gives rise to a new polyedron; the faces of which, becoming level with each other in pairs, are reduced to twelve, instead of twenty-four. The sulphuret of iron has assumed the form of a dodecaedron with pentagonal faces. But it is possible to conceive an infinite number of these dodecaedra, by varying the respective angles of incidence of the contiguous pentagons. What then is the dodecaedron of the sulphuret of iron? is it the regular pentagonal dodecaedron of geometricians? So two learned natural philosophers, Werner and Romé de l'Isle thought: but it is strictly demonstrated by algebra, that such a polyedron cannot result from any law of decrement. The angle of incidence between two contiguous pentagons at a given edge common to both alone determines all the other angles; and it is demonstrated algebraically, that, in the case of decrement of which we are speaking, this angle must be 126° 52′ 8″. Now, on measuring with the goniometer the angle that occurs in the sulphuret of iron, it is found to be nearly 127°: and from this agrement of the calculation with what is actually observed I infer, that the existence of the law of decrement is confirmed. Such is the rigorous method, in which Mr. Haüy constantly proceeds, when he applies his theory to the structure of crystals, to determine species in mineralogy.

Dodecaedral sulphuret of iron.

Not the regular dodecaedron, as supposed by Werner and l'Isle.

II. *Meionite.*

II. *Meïonite.*

Meïonite.

Mr. Mohs has attempted to raise doubts respecting the primitive form of the meïonite, which he is desirous of assimilating with that of the feldspar: but recent observations made on specimens lately brought from Vesuvius, very well marked and of a good size, have confirmed the angles, both of the primitive and secondary forms, to be the same as given by Mr. Haüy in his Treatise on Mineralogy. This gentleman, having broken crystals of this substance, has perceived joints parallel to the base, the position of which was at first merely conjectured. These joints, it is true, are not so clear as the lateral joints; but this is agreeable to the theory, which, giving a more extensive surface to the bases than to the sides, explains why the sections parallel to the bases are less easy to hit upon than those of the sides, where the points of contact are fewer.

Other characters beside form employed by Haüy.

I have yet compared the meïonite with the feldspar only in respect to form; but there are other characters, such as specific gravity, hardness, lustre, fusibility, &c. The method of Mr. Haüy, which is not purely orictometrical, far from excluding these, calls them in to the assistance of the geometrical characters in determining the species. Now Mr. Mohs says*, if the crystalline forms appear to militate against the union of the meionite with the feldspar, the other characters taken together will not allow us to part them: otherwise the method ceases to be natural, since it separates what nature has united.

The resemblance in these not so great as asserted.

I shall not stop here to discuss the greater or less resemblance ascribed to the physical or chemical characters of the meïonite with those of feldspar; a resemblance, which does not appear to me so great as is said; for on the one hand the meïonite is strongly scratched by many pieces of feldspar, and on the other, the latter does not melt before the blowpipe like the former with ebullition accompanied by a hissing noise, as has been observed by Mr. Leliévre, member of the council of mines, who is known to be very

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* Page 16 of the paper already quoted.

expert

The form alone sufficient to decide the point.

expert in this kind of proof. In the present case, the character borrowed from the form is sufficient. In fact, according to Mr. Mohs, only one primitive form can exist in a species: but the primitive forms of the feldspar and the meïonite are distinguished from each other in all the forms with which we are acquainted: their dimensions have been ascertained by a rigorous theory, the accuracy of which is proved by the agreement of calculation with experience. These alone therefore suffice to distinguish the species: if they did not, they might agree with other species, the forms of which would be different, and then one species would have two different forms, which is contrary to the hypothesis, and implies a contradiction.

Principle of Haüy's method.

Here we see clearly what distinguishes the method of Mr. Haüy. It is founded on the smallest member of characters possible. That which is taken from geometry, which is precise, is always employed, and frequently alone. When the primitive form obtained by mechanical division is a limit, that is to say, a regular, or at least a symmetrical solid, some other character must be added, since it may agree with several species.

The geometrical character not necessary to the student.

However, it is not necessary to determine the molecule of a mineral, in order to find to what species it belongs. This is a labour requisite only to the author of the method, who cannot employ means too precise for the determination of species. He whose object is merely to ascertain the species of a mineral, will find in the method of Mr. Haüy more manageable characters, that will guide him to his end.

Conclusion.

From the details into which I have entered it will be evident to all, who are acquainted with the theory of Mr. Haüy, that the forms of the meïonite are incompatible with those of feldspar, that the integrant molecules of the two differ essentially from each other, and, in fine, that these two substances ought to remain separate in the mineralogical system.

IX.

IX.

Method of finding the Quantity of Refraction from the Distance and Altitude of two known Stars; and of solving by Construction a Problem in Spherical Trigonometry. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

New method of finding the quantity of refraction.

THE following method of finding the quantity of the refraction by observing the distance and altitudes of two known stars is, as far as I know, new: and as it seems to possess some advantages over the common methods, I will venture to request its insertion in the Philosophical Journal.

Let Z, Pl. VI, fig. 8, be the zenith, S and X the apparent, s and x the true places of the stars.

Let d be the difference between their true and apparent distance; then S s the refraction of the star S $= \frac{d \times \text{tang. } ZS \times \text{cos. } MS \times \text{cos. } MX}{\text{S. } SX \times \text{rad.}^2}$

Demonstration.

Demonstration.

It is evident that $d\ (= Xm + Sn) = \frac{\text{cos. } \angle X \times Xx + \text{cos. } \angle S \times Ss}{\text{rad.}}$ (xm and sn being perpendicular to S X); but $Ss : Xx :: \text{tang. } ZS : \text{tang. } ZX \mathrel{.\dot{.}.}\ Xx = \frac{Ss \times \text{tang. } ZX}{\text{tang. } ZS}$; hence by substitution we get $d = Ss \times \frac{\text{cos. } \angle X \times \text{tang. } ZX + \text{cos. } \angle S \times \text{tang. } ZS}{\text{tang. } ZS \times \text{rad.}}$; but $\text{cos. } X = \frac{\text{cotang. } ZX \times \text{tang. } MX}{\text{rad.}}$ and $\text{cos. } S = \frac{\text{cot. } ZS \times \text{tang. } MS}{\text{rad.}}$; hence by substitution, $d = Ss \times \frac{\text{tang. } MX + \text{tang. } MS}{\text{tang. } ZS}$; but sum of tang. : S. of sum :: rad.^2 : □ of cos. *i. e.* tang. M X + tang. M S : S. S X :: R^2: cos. M X × cos. M S; hence $d =$

$$d = \frac{S\,s \times S.\ S\,X \times \text{rad.}^2}{\text{cos. } M\,X \times \text{cos. } M\,S \times \text{tang. } Z\,S} \text{ and } S\,s = \frac{d \times \text{tang. } Z\,S \times \text{cos. } M\,X \times \text{cos. } M\,S}{\text{rad.}^2 \times S.\ S\,X}$$

Q. E. D.

Choice of the stars.

The stsrs should be chosen so as to make the angles S and X acute, as the cos. of an obtuse angle would be negative.

Advantages of the method.

The advantages which this method seems to possess over those which are already in common use, are, 1st, that only one observation is required, as the refraction may vary considerably in the interval between two observations; and 2d, that it does not require the latitude to be known, and that the observation may be taken at sea with the instruments already in common use for lunar observations.

Problem. The sides of a spherical triangle given to find its angles.

The following method of solving by construction a problem in spherical trigonometry may possibly be new, and worth your insertion.

Given the sides of the spherical triangle Z S X, to find an angle Z. Let M I and M V (fig. 7) = the secants of the sides Z S and Z X, including the required ∠, take the ∠ I M V = the remaining side; let I Z, Z V = tangents of the sides Z S and Z X, and the ∠ I Z V will be = the required ∠ Z.

Demonstration.

Demonstration.

Let M (fig. 6) be the centre of the sphere, join M Z, M S, M X; draw Z T and Z V tangents to Z S and Z X; hence M T and M V are the secants of those sides, the ∠ T Z V = S Z X, and the ∠ M = the side S X. Hence supposing the triangle T Z V to come into the same plane with T M V, the two triangles will coincide with fig. 7.

Q. E. D.

Extension of the problem.

It is evident, that the cases of two sides and an included angle being given to find the third side, and of two angles and the side included to find the third angle, may be solved by a similar construction.

Yours, &c.

J. B.

X.

Torricellian Vacuum.

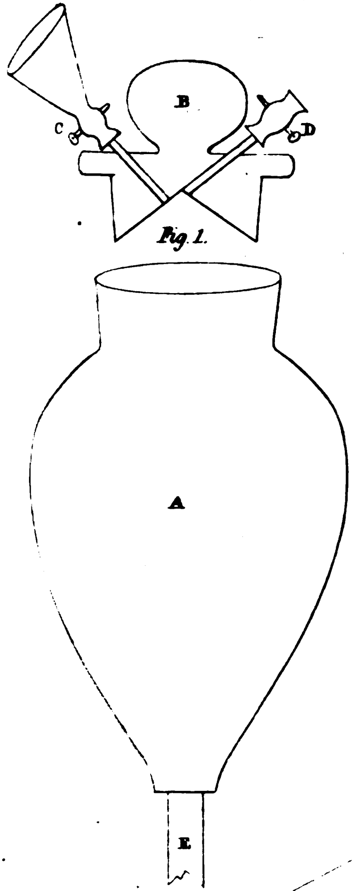


Fig. 1.

Feldspar

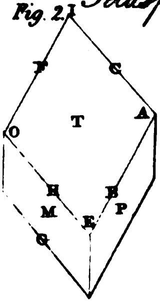
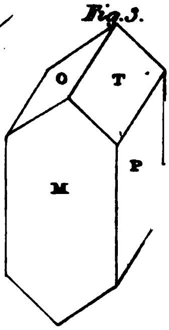
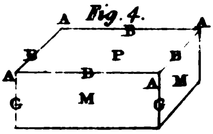
Fig. 2.


Fig. 3.


Meionite

Fig. 4.


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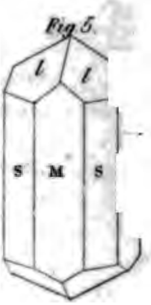

Problem in Trigonometr

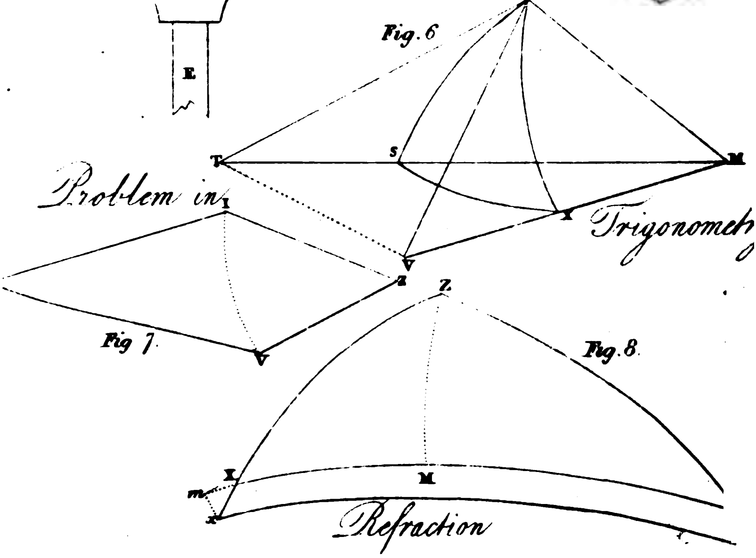
Fig. 6
Fig. 7.
Fig. 8.

Refraction

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X.

Early Account of an Albiness. In a Letter from JOHN BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

Early account of a female albino.

IN examining lately one of the earlier volumes of the Philosophical Transactions, I meet with the following account of an albino, which, as the subject has lately engaged your attention, I have transcribed for insertion in your Journal. It may be thought curious, both as being perhaps the earliest account on record of this peculiar variety of the human species, and also as furnishing another example of its occurrence in a female. It is taken from a paper on monsters, in the 25th volume of the Transactions, for the years 1706, and 1707, bearing the following title. "De monstris, quasi monstris, & monstrosis; item de serpentibus, & Phillippensibus, ex M. S. R. P. Geo. Jos. Camelli. Communicavit D. Jac. Petiver, Pharmacop. Lond. & S. R. S." It is divided into sixty-nine sections, each of which contains a narrative of some uncommon or monstrous production. The account of the albino is placed under the head of "Monstra quæ existebant, A. D. 1700 in insula Catanduen." "Albinam, Hispanis Albinno, vidi Manillæ; erat puella decennis, (proles Morenorum parentum, qui coloris sunt fuliginosi, sed capillitio protenso) albedinis extraordinariæ & insolitæ in admirationem trahentis, & monstruosæ, capilli aureoli, solem ac lucem invitæ ferens. Causam vulgus non phantasiæ sed lunæ influxui tribuit*."

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* "Monsters existing in the year 1700, in the island of Catanduanes.

"I saw at Manilla an albiness, called by the Spaniards an albinno. She was a girl ten years of age, the daughter of negrello parents, who are of a sooty complexion, with very long hair. She had an extraordinary, uncommon, wonderful, and unnatural white skin, golden hair, and was impatient of sunshine or light. The common people ascribe this not to the imagination of the mother, but to the influence of the moon." C.

If

Extraordinary tale.

If we except the concluding hypothesis, the account is probably correct; the extraordinary whiteness of the skin, and the great sensibility to light, are well characterized, and precisely resemble what fall under our own observation. It is indeed upon the antecedent probability of the narration, and not upon the credibility of the narrator, that we are to ground our belief; for many of the stories are palpably false and fabulous. The following may be taken as a specimen. A white woman brought forth a child the colour of a negro; the prudent midwife suspecting it to be the effect of some unsatisfied longing of the mother, found upon inquiry, that she had longed for some sardines (a peculiar kind of fish) that she had seen eaten by a black woman. Taking therefore the bones and remains of the fish, she rubbed them over the mouth of the infant, and immediately the dark colour was removed, and a white complexion produced*.

I am, Sir,

Your obedient Servant,

Clayton-Square, Liverpool, J. BOSTOCK.

OCT. 2d, 1808.

XI.

Remarks on the Doctrines of Chance, in Answer to Opsimath, in a Letter from W. SAINT, *Esq.*

To Mr. NICHOLSON.

SIR, *Woolwich, Sep.* 19, 1808.

I Read in the 91st number of your Philosophical Journal the letter of Opsimath, containing his "scruples as to the truth of the elementary doctrines of chance," should you think the following remarks upon the subject likely to remove the doubts of your correspondent, you will, by their insertion, oblige

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* I am inclined to think this account not altogether fabulous, though the assigned cause, and the remedy prescribed, are both palpably absurd. In some instances of difficult labour, the face of the child is so black, lips swelled, and nose flattened, that when born it resembles a young negro; but these appearances soon go off of themelves. Such was probably the case here; and the sagacious midwife applied her remedy time enough, to give it the credit of effecting the removal of what had probably excited astonishmnet and alarm. C.

Your very humble Servant,

W. SAINT.

Sense of the first case of de Moivre.

His sense mistaken.

The actual doctrine of de Moivre.

Opsimath begins by quoting what he deems to be the *sense* of the first case of de Moivre in these words, "Any "one undertaking with a die of six sides, to cast an ace in one "throw, has $\frac{1}{6}$ of the six possible chances in his favour, and the "remaining $\frac{5}{6}$ against him; *the whole six chances being cer-"tainty or at least such in the event of continued trials.*" Now this latter clause of the supposed quotation (I say *supposed* quotation, for Opsimath confesses, that he had not a copy of the work at hand), is not to be found in the first case of de Moivre, or yet in any other case; neither can it be inferred from any thing which he has said on the subject throughout the whole of his work. Indeed had Opsimath proceeded but a few pages farther than the first case, he would have seen, that it was impossible for de Moivre to have considered this as an elementary doctrine of chance, for at Art. 11 he says, "Let a be the number of chances for the happening "of an event, and b the number of chances for its failing, "then the probability of its happening once in any number of "trials will be $\frac{a}{a+b} + \frac{a\,b}{\overline{a+b}|^2} + \frac{a\,b^2}{\overline{a+b}|^3}$ &c., till the "number of terms be equal to the number of trials given:" the application of which would give $\frac{31031}{46656}$ for the probability of throwing an ace once in six throws; whereas Opsimath infers, and infers justly, from the expression which he attributes to de Moivre, that $\frac{6}{6}$ or certainty would be the amount of the probability; and this single circumstance, had Opsimath proceeded so far, would have convinced him, that he must either have attributed that to de Moivre which he had never asserted, or else, at least, that he himself must have misunderstood him.

Since the clause abovementioned appears to have been the foundation of Opsimath's scruples, and since this clause is not to be found in de Moivre, or I may add in any other author that I have seen on the Laws of Chance, perhaps to say any thing farther on the subject may be deemed unnecessary.

Farther statement of doubts.

Answer to these.

Lest however the doubts of Opsimath should not be fully removed, let as proceed with him a little farther.—He goes on to say, or rather to quote, that "any one undertaking to "cast an ace in two throws of one die, has for the first probability $\frac{1}{6}$, as proved: should the first fail, then the second "remains, which is $\frac{1}{6}$ likewise; but the chance of the first "failing is $\frac{5}{6}$, as that of its succeeding is $\frac{1}{6}$; therefore the second throw has only $\frac{1}{6}$ of $\frac{5}{6}$ for its chance of success, which "added to the chance of casting an ace the first throw, is $\frac{1}{6}+\frac{1}{6}$ "of $\frac{5}{6}=\frac{11}{36}$; the first throw being $\frac{6}{36}$, the second only $\frac{5}{36}$." "This doctrine," Opsimath adds, "I cannot grant"—"because" says he, "nothing can prevent him of the second throw, except his succeeding in the first." Very true, but his succeeding in the first *may* and certainly *will* prevent him of the *second* throw. Opsimath should recollect, that the probability of the event's happening is calculated *before* either throw is made; and that, till the *first throw is made*, it is uncertain whether the *second* will be required; and consequently, that, though the second throw has "the full "force and virtue of $\frac{1}{6}$ chance" *after the first is over*, yet, *before* that event, its value can only be $\frac{1}{6}$ multiplied by the probability that the first throw will *fail*, for on the *failure* of the first depends the necessity of the second—that is, since the probability of the *existence* of the second throw, if I may so term it, is, before the first takes place, only $\frac{5}{6}$; and, *should it exist*, the probability of its producing an ace is only $\frac{1}{6}$; therefore, before either throw is made, the value of the probability of the second is only $\frac{5}{6}$ of $\frac{1}{6}$, that is $\frac{5}{36}$, which, added to $\frac{1}{6}$, the probability for the first throw, gives $\frac{11}{36}$ for the probability on the two.

Answered by deducting the probability of failure.

Should this consideration of the *dependence* of the second event upon the first fail to remove the scruples of Opsimath, yet, I think there will be no difficulty in convincing him upon the principles, which he has himself admitted, that $\frac{11}{36}$ express the true probability of casting an ace once in two throws. Since the probability of an event's happening, together with that of its failing, makes certainty, which is represented by unity, or 1, therefore the probability, that there will be an ace in one or two throws, together with the probability that there will not be one, is equal to unity. Now the probability, that there will *not* be an ace the first throw is $\frac{5}{6}$, and since, whether there be one or not, the second throw will be equally necessary for determining the probability that there will not be one in *either throw*, therefore this second throw exists with "the full force and virtue of the first, from which no circumstance can deduct," viz. the probability in the second throw will be $\frac{5}{6}$, therefore the probability that there will *not* be an ace in either the first or second throw will be $\frac{5}{6} \times \frac{5}{6} = \frac{25}{36}$, and, deducting this from unity, there will remain $\frac{11}{36}$ for the probability that there *will* be an ace in either the first or second throw, the same as before.

Perhaps there is no branch of the mathematics, which is founded upon fewer first principles than the Laws of Chance; and yet probably there is no subject, the first principles of which are so likely to be misapplied, or misunderstood. One of the principal causes of the errours in our reasoning on this subject, it may safely be affirmed, is the not duly discriminating between events which are dependent and those which are independent; and this seems to have been the source, whence the scruples of Opsimath have originated; scruples, which, it may be asserted, have been entertained by almost every one on his first entrance on this subject. Perhaps, however, what has been said above may tend to remove these doubts, if not, we will conclude by advising Opsimath to acquire correct ideas of the first principles of the Laws of Chance; and if in his inquiries he be guided by right reasoning, we will assure him, that there is no subject in which he will find the conclusions more just, natural, or beautiful.

Laws of chance founded on very few principles, yet very liable to be misunderstood; chiefly from not discriminating between dependent and independent events.

As it often happens, that theory is best understood by practice, and precept best illustrated by example, I have enclosed the following question, taken from the Mathematical Repository, which, as it is rather of a curious nature, you may perhaps deem worthy of insertion; and as the solution, which I have given to it, is founded upon the first and most obvious principles of the Laws of Chance, it may probably be useful not

Curious question elucidating the doctrines of chance.

not only to Opsimath, but to many other of your correspondents.

Question.

A, with a truths, tells b falsehoods, and B with c truths, d falsehoods, what is the probability of the truth of a circumstance in which they both agree?

Solution.

Probability of the truth of a fact related by two persons.

As they are supposed to *agree* in their relation, they must either *both* speak truth or *both* falsehood. Since then the probability of an event's happening is always expressed by the quotient of the number of times in which it may both happen and fail, it is evident, that in the present case, the probability of a circumstance being true, in which both of them agree, will be expressed by the quotient of the number of times in which they may agree in telling truth divided by the number of times in which they may agree in telling both truth and falsehood. Now the number of times in which they may agree in telling truth will be the number of combinations of a in c, viz. $a\,c$ (for each of the truths a may be told with each of the truths c); and for the same reason the number of times in which both of them may agree in telling falsehood, will be $b\,d$; the true expression therefore for the probability required will be $\frac{a\,c}{a\,c+b\,d}$.

Cor. 1.

Cor. 1. Let a, b, c, and d be all equal, then will $\frac{a\,c}{a\,c+b\,d} = \frac{1}{2}$, now the probability of A's telling truth is expressed by $\frac{a}{a+b}$, which will also $= \frac{1}{2}$; hence the probability of the truth of a circumstance in the relation of which two persons agree who are each in the habit of relating truths as often as falsehood, will be the same as if related by either of them separately.

Cor. 2.

Cor. 2. If a be greater than b and c greater than d, then will $\frac{a\,c}{a\,c+b\,d}$ be greater than either $\frac{a}{a+b}$ or $\frac{c}{d+d}$; for $\frac{a}{a+b} = \frac{a\,c}{a\,c+b\,c}$ and $\frac{c}{c+d} = \frac{a\,c}{a\,c+a\,d}$ Now since a is greater than b, and c greater than d, the denominators $a\,c+b\,c$ and $a\,c+a\,d$ to the common numerator $a\,c$ will be each of them greater than $a\,c+b\,d$ and consequently the value of the fractions less than $\frac{a\,c}{a\,c+b\,d}$, viz. the probability of a circumstance being true in the relation of which two persons agree, who are each in the habit of relating more truth than falsehoods, will be *greater* than when related by either of them separately.

Cor. 3. If a be less than b and c less than d, then $\frac{a\,c}{a\,c+b\,d}$ Cor. 3.
will be less than either $\frac{a}{a+b}$ or $\frac{c}{c+d}$; viz. the probability of the truth of a circumstance in the relation of which two persons agree, who are each in the habit of relating fewer truths than falsehoods, will be *less* than when related by either of them separately.

Cor. 4. If a be either greater or less than b, and $c=d$, Cor. 4.
then will $\frac{a\,c}{a\,c+b\,d}=\frac{a\,c}{a\,c+b\,c}=\frac{a}{a+b}$; viz. the probability of the truth of a circumstance in the relation of which two persons agree, the one of whom is in the habit of relating an equal number of truths and falsehoods, and the other any number of truths with any other number of falsehoods, will be the same as if related by *that other only*.

Cor. 5. If $d=o$, then $\frac{a\,c}{a\,c+b\,d}=\frac{a\,c}{a\,c}=1$ or certainty, Cor. 5.
viz. the probability of the truth of a circumstance in the relation of which two persons agree, the one of whom uniformly relates truth, and the other any number of truths with any number of falsehoods, will always amount to certainty: as is evident from reflection also, for, in this case, *to agree* they must *both* speak truth.

Cor. 6. Hence also it appears, that the corroborating tes- Cor. 6.
timony of a second person or witness is not *always* an additional evidence in favour of the truth of a circumstance related by the first, for if d be greater than c, $b\,d$ will be greater than $b\,c$, and consequently $\frac{a\,c}{a\,c+b\,c}$ $\left(\text{or its equal } \frac{a}{a+b}\right)$

will be greater than $\frac{ac}{ac+bd}$, viz. the probability of the truth of a circumstance is greater if related by *one person only*, than if related by *two*, when the second is in the habit of relating a greater number of falsehoods than truths.

Cor. 7.

Cor. 7. Lastly, if the relations be supposed to be *untold*, or, in other words, if it be supposed that A and B are each *about* to relate a circumstance, the probability that they will both speak truth will be expressed $\frac{ac}{\overline{a+b} \times \overline{c+d}} = \frac{ac}{ac+ad+bc+bd}$; for the probability of A's speaking truth would be $\frac{a}{a+b}$, of B's $\frac{a}{c+d}$ and therefore of both $\frac{ac}{\overline{a+b} \times \overline{c+d}}$.

XII.

Farther Remarks on the Doctrines of Chance. By OPSIMATH.

To Mr. NICHOLSON.

SIR, *October 7th*, 1808.

Not convinced by a correspondent's arguments;

I Beg to thank your correspondent Mr. B. H. for his remarks on my letter respecting the Doctrines of Chance, obligingly inserted in your Philosophical Journal for September, although not productive of conviction on my judgment:—they strictly conform with the systems of de Moivre and Thomas Simpson, whose publications are the only works on this subject, which I have seen. But as I fear not to have made the path of reasoning, which leads to my deduction, as plain as it admits, I shall attempt to do so more effectually now, provided my humble essay does not intrude on pages dedicated to the promulgation of so much more valuable information.

and why.

The variation of result arises, as Mr. C's remark observes, from six successive throws of one die being assumed equal to 1 simultaneous throw of 6 dice; which position, in my mind, it completely subverts, though supported by the authority of the above celebrated names. Let us compare 2 throws of 1 halfpenny with 1 throw of 2, as to their chances of a head's being thrown, which are less complex, and stand on precisely the same base with the throwing of 2 dice.

Case of a halfpenny.

In the former case I say, I have ½ of success as my first probability; if successful, *I dispense with the second throw, which is however altogether optional on my part, being my privilege by premises.* If unsuccessful in the first, I of course avail myself of the second chance, which, when to be exercised, I cannot estimate in any wise less valuable than its predecessor; and thus I have in all 2 one half chances of success equal to each other, and together equal to assumed certainty on the average of probability: at least such is my conclusion, for I cannot lose without first having had 2 one half chances of winning.

In the latter case I say, I can only lose by throwing 2 tails at once: the probability of throwing one of the halfpence a tail is evidently ½, and of doing so with the other, were this effected, ½ also; therefore the contingency of throwing both tails, is ½ of ½ = ¼. Now the probability of failing ¼, being deducted from unity, or assumed aggregate of all chances, leaves ¾ for the probability of succeeding. Or otherwise, as I can win by throwing 2 heads, for which I have ¼ probability, and also by throwing 1 head, for which I have ½ probability, the amount of probabilities to do one of them, is as before ¾.

Therefore I estimate 2 throws of one halfpenny, ¼ better than 1 throw of 2 halfpence in the chance of throwing a head.

If required to throw two heads.

But if it were required to throw 2 heads instead of 1 in the above cases, I estimate the chances of 2 successive throws of one halfpenny, and of 1 simultaneous throw of two halfpence, perfectly alike, viz. each ¼; for in this instance, each of the 2 heads supposed to be thrown at once with the 2 halfpence has its value; in the former 1 head is without value at all. And here stands the deceptive point of distinction, the combination of 2 aces with dice, as pointed out by C.

If the value of one throw be decreased, the other ought to be diminished.

But reasoning even with the disciples of de Moivre, I cannot but observe, if they diminish the value of the second throw of 1 die, they ought proportionally to increase the value of the first; for it strictly yields them a twofold advantage, viz. ½ chance of success as admitted, *and likewise* ½ chance of another probability on the failure of that.

And Mr. B. H. advancing the *entire coincidence of probability* of the 2 dice with one throw, and of the 1 die with 2 throws, as he gives No. 1 throw, $\frac{1}{36}$ advantage over No. 2 throw, he can not in justice withhold from die A, the same $\frac{1}{36}$ advantage over die B, when thrown together; which is exactly the fatal invalidity of its ace, in combination with the ace of A.

I remain, Sir,

Your obliged, and most obedient Servant,

OPSIMATH.

REMARK.

Too many letters sent to be admitted.

NUMEROUS communications of considerable extent, and some of a controversial nature, having been received on the Doctrines of Chance, it was impracticable for the editor to insert them all, notwithstanding the merit of several, as they would have occupied a great deal more room than is consistent with the plan of his work. He has however admitted the letter of Mr. Saint, as containing a curious problem in the application of the doctrine of chances; and has thought it right, that Opsimath should again be allowed to speak for himself.

The word certainty taken in a loose sense.

In answer to the latter gentleman, he would observe, that he appears to be misled by not adhering to the strict meaning of the word certainty, and confounding it with what may properly be termed the right of expectation. In throwing a die, there is no reason we can assign, why a deuce, a trois, or any other of the sides, should turn up preferably to an ace. We have therefore a right to expect, that an ace will be turned up once in six times. Farther, if I do not throw an ace the first time, when I have to throw a second, I have neither more nor less chance of bringing an ace, than I had the first time. Thus, if a stake of thirty guineas were deposited, to which the thrower of an ace would be entitled, I ought to give five guineas for the throw, it being just one fifth of what I should win, and there being one chance for my winning, and five for my losing. If I lost, and chose to throw again, I ought again to give five guineas for the throw, as my chance would be precisely the same; and so on for any single throw, however often I might fail. Still, though previous to my having thrown at all I should have a right to expect to throw an ace in six throws, it is not a certainty, for I might very possibly throw some other number every time. In fact, no sum of contingencies, make them as great as we please, can ever amount to a certainty, unless we take all the chances both for and against a thing's happening: And certainty is used with strict precision in the doctrines of chance, as being the sum, not of all the chances of success alone, or of failure alone, but of all the chances both of success and failure. Thus if I had a box capable of throwing ten thousand dice at once, and were to throw them ten thousand times, however great the probability of bringing an ace out of the hundred millions of faces, it would be by no means certain; for ten thousand dice admit of a great variety of combinations, in which no ace appears, and one or other of these combinations might turn up each of the ten thousand times. Now, the beauty of the doctrines of chance consists in this very thing, that they appreciate, not merely what we have a right to expect, in any given instance, but the chance there is of our failing of this expectation. We have a right to expect an ace in six throws of a die. If we throw a greater number of times, we have a right to expect one sixth of the number will produce aces: and the greater the number of times, the nearer the number of aces will be likely to approach to one sixth of the whole; since it is obvious, that there will be the greater chance of more aces than one turning up in some of the series of six successive throws to compensate for those series of six in which none have occurred. Now these probabilities the doctrines of chance, as established by some of the ablest mathematicians, calculate with much precision on solid principles: and it is in this way we find, that, though we have a right to expect to throw one ace in six throws of a die, yet the chance of so doing is $\frac{15625}{46656}$ worse than certainty.

No sum of contingencies amount to certainty.

Doctrines of chance estimate probabilities with precision.

I cannot conclude without observing, that there is considerable merit in a student's refusing implicit reliance on any name, however great; and suspending his judgment till his understanding is convinced; but I trust what has been said will prove sufficient, to remove the doubts of Opsimath. C.

XIII.

Memoir on the Organ by which the fertilizing Fluid is capable of being introduced into the Ovula of Vegetables. By P. Turpin. *Read at the National Institute, December the 4th,* 1808*.

Discoveries owing to chance, or reasoning & examination.

IN Natural History, as in all other sciences, we are sometimes indebted to chance for discoveries, though they more frequently arise from the deductions of reasoning, and from observation. It is to the last of these I owe the discovery of the organ, which will be described in this paper. This organ, hitherto noticed only in the seeds of the leguminous plants by those celebrated botanists Grew, Gleichen, and Gærtner, and in our own days by Mirbel, according to my researches forms a necessary part of the structure both of monocotyledonous and dicotyledonous seeds.

Coats of a seed.

Before we proceed let us examine what are the principal organs, that the two coats of the ovula exhibit; or, as the readiest way, let us examine the proper coats of a seed arrived at maturity.

Base of a seed contains three different organs.

It is admitted, that the base of the seed, whatever its figure, is always determined by the point which adheres to the placenta. This point, which has received several names, such as umbilicus, hilum, and eye, comprises three distinct organs, each having a different function to fulfil, yet all hitherto confounded by botanists under one term.

The hilum.

The first of these organs, to which the name of hilum is perfectly adapted, is that cicatricula, which is most commonly called the umbilicus of the seed. The lips of this cicatricula, which are sometimes very large, as in the sapota plum, soap-berry, chesnut, and some legumes, inosculate with the exterior vessels of the umbilical cord, which, dividing afterward throughout the whole extent of the outer coat, constitute its vascular organization.

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* Journal de Physique, vol. LXIII, p. 195.

The omphalodes.

The second, which I term omphalodes*, is an aperture placed most commonly in the centre of the hilum, but sometimes toward one of its extremities, and sometimes it is a longitudinal cleft extending from one end of it to the other. This organ, wholly negleted by botanists, forms the passage between two other vascular systems; the first of which, that is the outermost, after having inosculated with the lips of the hilum of the internal membrane, forms its organization in the same manner as that of the outer coat already noticed. In fine, as we observe an omphalodes on the outer integument, we perceive one on the internal membrane, through which the third vascular system passes, consisting of the umbilical vessels, by which the embryo was attached to the parent plant previous to its fecundation, and for some time after†.

The third is the subject of the present inquiry.

Direction of the radicle.

All physiologists are aware, that the point by which the ovula adhere to the ovaries marks the direction in which the radicle will push forth, and this is without exception. For instance in some families of plants, as the dipsaceæ, caprifoliaceæ, and jasmineæ, the ovula are constantly attached to the summit of the cavity of the ovaries, and the radicle is superior: in others, as the campanulaceæ and composite, the point of adhesion is inferior, and the radicle is the same. But the better to generalize our ideas, let us rather say, that the direction of the embryo is always subordinate to that of the seeds in the pericarp, and that the point by which these are attached always determines the direction of the radicle*.

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* From the greek ομφαλος, the navel, and οδος, a way.

Umbilical vessels.

† Grew appears to be the first, who observed the umbilical vessels of the embryo. These umbilical vessels, the only ones that deserve the name, constitute the innermost vascular system, which, after having passed the coats of the seed by means of the omphalodes, divides into two branches, each of which inosculates with the lobes of the embryo, near the point where they unite with the radicle and plumula It is to be presumed, that these vessels quit the young plant pretty early; for it is extremely difficult, to find any traces of them in ripe seeds, except in those of some of the coniferous plants, the tropæolum, and several of the legumes, in which the two umbilical cicatriculæ are very evident.

Point of attachment of the ovulum.

It is known too, that the point of attachment of an ovulum is the umbilicus, with which an infinite number of vessels, destined to form at first the vascular organization of all parts of the seed, and then to convey nourishment to it both before and after fecundation, inosculate in the form of a cord of greater or less length: but how is this fecundation effected? by what way can it reach and penetrate the ovula. This is certainly an important question to be solved, and on which, to this day, scarcely any thing has been said. The opinion most generally received is, that the prolific vapour descends from the papillæ of the stigma into the placenta, and transmits the fecundation to the embryo through the umbilicus. But I would here appeal to reason, and ask whether it be conceivable, that the same vessels, and the same aperture on the ovula, can fulfil two such different functions as those of conveying to the embryo nutrition and fecundation, the sources of which are so opposite.

Fecundation.

Common opinion respecting it.

Not probable.

Another organ sought after and discovered.

Such was the reasoning that induced me, to examine carefully whether some other organ beside the nourishing umbilicus did not exist in the ovulum. It was not long before I discovered what I at first suspected: for on the first dissection I observed near the cicatricula of the hilum another aperture, which I could not avoid immediately considering as the organ, by which the intromission of the fertilizing vessels must take place.

This organ always as near as possible to the eye,

This organ, as I have satisfied myself by more than twelve hundred dissections of seeds with one and two cotyledons, is always placed as close as possible to the hilum at the time of fecundation; and if it sometimes recede from it afterward

Direction of the radicle.

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* When I say, that the radicle is always directed toward the umbilicus, I mean the umbilicus of the internal membrane. This membrane, to which the direction of the embryo is always subordinate, may sometimes be inverted in the outer integument, as in the lousewort and eyebright: for as there are seeds inverted in the pericarp, for instance in the plum and the hazel nut, so it happens, that the interior membrane is inverted in the outer. This organization requires, that the umbilical cord, after having passed through the exterior omphalodes, should creep between the two coats, to inosculate at the base of the interior membrane, which in this case is opposite to that of the exterior.

terward, it is solely owing to the growth and enlargement of the seed. Its situation near the point of adhesion is such, that the fertilizing vessels may enter it by the shortest way. Thus in the labiati we find it constantly placed toward that part of the hilum, which faces the centre, and is consequently as near as possible to the style. In the liliaceous and leguminous plants, and in general all those that produce capsules in which the seeds adhere laterally, it is superior to the point of adhesion, as we may easily see in the French bean, or any other pulse. I ought also to observe, that it constantly answers to the point of the radicle*, in every seed in which the internal membrane retains the same direction as the outer integument. If on the other hand we consider, that the fertilizing vessels can have no other communication with the embryo but by the papillæ of the stigmata; and if to this be added, that the fecundation is intended solely for the embryo, and influences it alone, which it would be easy to prove by a number of facts; we shall not be surprised, that there are two entrances to the ovula, the first of which, termed by me the micropyle†, serves to give a passage to the fertilizing vessels, while the second, being the umbilicus for conveying nourishment, must be intended for the inosculation of the sapvessels of the parent plant. The sole function of the latter must be that of supplying aliment suited to the delicacy of the young embryo, by furnishing it with juices already in some sort digested and filtered by the extreme tenuity of these vessels.

and opposite the point of the radicle.

Two distinct passages, one for the fertilizing, the other for the nourishing vessels.

The existence of fertilizing vessels has long been proved. They have engaged the attention of physiologists ever since the establishment of the Linnean system; several have traced them from the stigmata to the ovula; and they believed,

Fertilizing vessels.

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* Every physiologist knows, that the radicle is the part of the embryo, in which the vital principle appears strongest This part, which is always the first perceived after fecundation, is likewise that which first lengthens and dilates in germination: accordingly it is toward this, that nature has thought proper to carry directly the fertilizing fluid, placing it opposite the micropyle, through which the vessels intended for this function enter

The radicle.

† Micropyle, from μικρος, small, and πυλη, a gate.

lieved, that these vessels, uniting with the umbilical cord, transmitted the fecundation to the embryo through the umbilicus itself. But as this cord is an assemblage of fertilizing and nutritive vessels, and as there are two apertures at the place where it reaches the ovula, is it not more reasonable to suppose, that it is divided there; that the nutritive vessels inosculate with the umbilicus properly so called; and that the fertilizing vessels pass through the micropyle, to communicate immediately to the embryo the vital principle, or rather that contact, so necessary to the first life of every organic being*?

The micropyle not easily seen in ripe seeds.

Grew saw it, but mistook its office,

and in another place ascribes a similar office to the coats.

The little perceptibility of the micropyle on seeds arrived at maturity is perhaps one of the causes, why it has been overlooked by so many natural philosophers. I said at the beginning, that it had been seen on several of the leguminous seeds by Grew, Gleichen, Gærtner, and Mirbel: but none of these expert observers, except Grew, deemed it of any importance. Grew ascribed to it two functions, one of which has been already refuted by a number of experiments made on the subject. In the first place he imagined, that this aperture might serve to facilitate the introduction of air and moisture into the seeds at the moment of germination. This notion, which might appear very ingenious and satisfactory when Grew wrote, is inadmissible in the present state of our knowledge. We now know from a thousand experiments, that the stopping up of this aperture, and even that of the omphalodes, with wax or varnish, does not prevent the developement of the embryo. Grew himself, in another part of his work, overturns the use he had at first ascribed to this organ, when he says expressly: "the bean "being enclosed in its skins, it is necessary, that the juices "intended for its nourishment must pass through them by "filtration, and impart to the embryo only the quantity re-"quisite. If the embryo were divested of these, it would "draw too much juice; and as it would be without its fil-"ters,

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Organized beings have two lives.

* Every organic being has two lives. The first receives its fertilizing principle and nutrition by means of an umbilicus. The second commences at the moment when the embryo or fœtus, having attained its appointed degree of maturity, separates from the placenta, and takes in aliment at a single mouth or at thousands.

"ters, which commonly strain the moisture like a very fine "cotton, it would perish, from being unable to feed on too "gross aliment." It is easy to perceive by this passage, that Grew contradicts himself, and that, admitting with more reason the use of the coats, which he very ingeniously compares to filters, he entirely rejects his first opinion of the functions of the micropyle.

He likewise supposed it afforded a passage to the radicle.

This improbable from its smallness,

and the flexion it would require in some cases.

This learned anatomist, having observed the micropyle only in a small number of leguminous seeds, in which this organ is constantly placed opposite the point of the radicle, had imagined, that it likewise served to afford this a passage in the process of germination. But how is it to be conceived, that a radicle twenty or thirty times as large as the aperture of the micropyle can issue through it? Besides, where is the person, that has ever had an opportunity of seeing a seed in the state of germination, who has not observed, that the radicle never emerges from its captivity, till the coats, being unable longer to contain the embryo, regularly burst, and thus give a passage first to the radicle, and afterward to the entire young plant? If on the other hand we add to this refutation, that, in a considerable number of seeds, the interior membrane describes a quarter of a circle round itself in the outer integument, as in the commelina and tradescantia, or a semicircle, as in the eyebright, lousewort, and cow-wheat, we shall plainly perceive, that the micropyle of the interior membrane, to which the point of the radicle answers, must be a quarter of a circle distant from the outer micropyle in the former, and half a circle in the latter; and that from this construction it would be impossible for the radicle ever to issue by this aperture, since for this purpose it must wind between the two coats, to come out at last through the external micropyle, which in seeds of this kind is always opposite to the micropyle of the inner membrane, and to the radicle, which is inseparable from the latter.

A new law in the science of fruits.

If I have been so fortunate as to make known the true way of fecundation in the ovula of vegetables, this is not the only advantage, that vegetable physiology will derive from my labours; for the dissections I have been obliged to make, to generalize the presence of the micropyle in all seeds, have enabled me to add a law to carpology, which I conceive to be of such a nature as to admit no exception.

Fruits consist of four distinct parts.

The last three may be considered as one.

Characters by which a seed may be distinguished from a pericarp.

The acorn, chesnut, and nut of the nelumbium, not seeds.

Thoroughly to understand this law, it is necessary to recollect, that all fruits are composed of four very distinct parts, each of whish has its own peculiar system of vessels. The first is the pericarp; the second, the outer integument of the seed; the third, the internal membrane; and the fourth, the embryo. But I conceive, that, to facilitate the study of carpology, it will be sufficient to divide fruits into two parts only; the first of these being that envelope of various forms, and of various substance, which botanists term the pericarp; and the second, the seed, which is always united by an umbilical cord to a central receptacle, detached or adherent, or to the inside of the pericarp. These two parts, which have been too frequently confounded together, may be discriminated in future by invariable characters easily distinguished. A seed must always be attached to an umbilical cord, longer or shorter, and always provided with two cicatriculæ at its base, one of which is the nutrimental umbilicus, the other the micropyle: but it cannot in any case have a style, since the styles themselves are nothing more than an elongation of the placenta, or receptacle. Thus the acorn separated from its cup, the chesnut divested of its bristly coat, the nut of the nelumbium taken out of its receptacle, cannot be seeds properly so called, since their coats are terminated by styles. It is undoubtedly for want of knowing this law, that Gærtner, after having described the acorn and chesnut as pericarps, describes the nut of the nelumbium as a simple seed*.

Recapitulation.

On considering what has been said in this paper, it appears, that the micropyle is constantly placed near the umbilicus at the time of fecundation; and that, if it afterward recede from it, this is owing to the dilatation of the seed:

The micropyle distinguishes a seed from an aril.

* The mycropyle may serve likewise to distinguish the seed from the aril. The latter, as Mr. Richard has very justly observed, being only an expansion of the umbilical cord, which covers the seed wholly or in part, cannot have the micropyle, the orifice of which is always in the proper coat of the seed.

that in all these seeds, in which the internal membrane preserves the same direction as the outer integument, its situation is always opposite to the point of the radicle: that the umbilical cord, or rather that assemblage of the nutritive vessels belonging to the coats of the seed and the embryo, cannot admit into it the fertilizing vessels: that the extent of these in the plant is and must be only from the papillæ of the stigmata to the embryo: that, after having descended into the placenta, they join the nutritive vessels, and then proceed with them, forming a single cord, to the point where the ovulum is attached: lastly, that at this point there are two apertures, and it appears probable, that the nutritive vessels pass through the umbilicus, and the fertilizing vessels through the micropyle.

Geoffroy observed this organ.

Note. When I wrote the above paper, I did not know, that the organ of which I was speaking had already been observed by Geoffroy, though it has not been mentioned by the authors who succeeded him.

His account of it.

Geoffroy's paper is inserted among those of the academy of sciences for the year 1711, and is entitled, Observations on the Structure and Use of the principal Parts of Flowers. The author recognises the existence of the micropyle in all seeds, and ascribes to it the same functions as I have done, but with some little difference. I conceive I cannot do better, than describe the passage, in which this gentleman, after having attempted to show, that every grain of the pollen might be a germe, destined to be introduced into the ovulum, and there become a young plant, says, p. 230. " Pursuing this conjecture, it is not difficult to ascertain in what way the germe enters into the vesicles: " for, beside that the cavity of the pistil reaches from its " extremity to the embryoes of the seeds, these vesicles " have likewise a small aperture near the place where they " are attached, which is at the extremity of the canal of " the pistil; so that the small particle of dust may naturally fall through this little aperture into the cavity of " this vesicle, which is the embryo of the seed. This cavity, or kind of cicaticula, is sufficiently evident in most " seeds: it may be seen very easily, without the assistance "of a microscope in pease, beans, and French beans." Here Geoffroy falls into the same mistake with Grew, when he adds; "The root of the little germe is quite close "to this aperture, and through this same aperture it issues "out, when the seed comes to germinate."

Progress made in vegetable physiology since his time.

When we reflect on what Geoffroy says, it is easy to perceive the progress, that has been made toward the knowledge of plants within a century. We can no longer suppose with this naturalist, that the particles of the pollen are germes, as he says; and still less can we think, that these particles can ever be introduced into the ovula by the micropyle. The present state of our knowledge instructs us, that the particles of dust contained in the anthers are so many little bladders filled with a fluid, the only substance to which we allow a fertilizing quality, and the only one capable of being conveyed into the embryoes.

We also know, that the canal found in the centre of the styles of all the monostyle ovaries, and destitute of a central adherent receptacle, cannot in any way promote the process of fecundation, and is nothing but the cavity of the ovary, which is prolonged through the style as far as the stigma.

XIV.

Essay on the Composition of Alcohol and of Sulphuric Ether, By THEODORE DE SAUSSURE. *Read to the Physical and Mathematical Class of the Institute April the 6th.* 1807*.

SECT. I. *Introduction.*

Proportions of the elements of vegetables little known.

THE proper methods of arriving at a knowledge of the proportions of the ultimate elements of vegetables are yet so uncertain, and so badly determined, that every inquiry into the subject must furnish useful observations, whatever be the material to which it is applied. The theory of fermentation can be known only by an analysis of its products, and among these alcohol will always hold an important place.

Fermentation.

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* Journal de Phissique vol. LXIV, p. 316.

Conversion of alcohol into ether.

Contradictory opinions respecting it.

Two ways of coming at the truth.

Analysis of the residuum of the ether too difficult.

Easiest method to analyse both ether and alcohol.

The change experienced by this fluid during its transformation into ether has occupied the attention of the ablest chemists. Some have ascribed to ether more oxigen and less carbon than to alcohol*: others have embraced the opposite opinion†. These contradictory conclusions are founded on indirect considerations, and the question must remain undecided, if it be not subjected to a more profound examination. This may be accomplished by two different processes. One consists in analysing the residuum left by the alcohol and sulphuric acid after the separation of the ether: but this residuum, which consists of several different and very compound substances, requires for its examination an immense labour abounding with difficulties. The other process confines itself to the analysis of alcohol and of ether, and to deducing from their difference the changes they have undergone during their transformation. I have chosen the latter mode: as to the advantage of being more easy it adds that of giving us a more absolute knowledge of the composition of these two substances.

Mode here pursued.

The operation by which I have analysed them consists principally in changing them, by an addition of oxigen, into water, and carbonic acid gas, and estimating from the known composition of these the quantities of carbon, oxigen, and hidrogen, contained in alcohol and in ether.

Elements of water and carbonic acid not completely ascertained.

The proportions of the elements of water and carbonic acid gas have not been ascertained with such precision, as to leave no uncertainty respecting them; and I will not venture to affirm, that those I have adopted, and which I am about to mention, are preferable to any other. It will be easy in this respect to alter the last terms of my analyses, considering, 1st, the volume of the oxigen gas, which I caused to disappear by burning a given weight of alcohol and of ether; and, 2dly, the volume of carbonic acid gas produced at the same time. These two terms alone are the fundamental and important expression of my results. In all the subsequent experiments I admit

Proportions of

1, that 100 parts of water contain 88 parts of oxigen by

weight,

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* Annales de Chimie, vol. XXIII, p. 43.

† Statique chimique, par Berthollet, vol. II, p. 532.

principles adopted in this paper.

weight, and 12 parts of hidrogen, neglecting the fractions.

2, that two parts by measure of hidrogen gas saturate one of oxigen gas, to form water.

3, that 1000 cubic inches of hidrogen gas, the barometer being at 28 inches, and the thermometer at 10° Reaumur [54·5° F.], at the point of extreme dryness weigh 34·303 grs*.

4, that 100 cubic inches of oxigen gas, under the same circumstances, but at the term of extreme moisture, weigh 512·37 grs.

5, that 1000 cubic inches of carbonic acid gas, under the same circumstances as the last, weigh 693·71 grs.

6, that carbonic acid gas contains its own bulk of oxigen gas.

7, that 100 parts by weight of carbonic acid gas at the point of extreme humidity, contain 26 parts of carbon, neglecting fractions†.

Alcohol at ·792 rectified from muriate of lime.

The alcohol I employed for this analysis was such as Lowitz and Richter designate by the name of *perfect alcohol*, and which they have instructed us how to prepare. Its specific gravity is 0·792 at the temperature of 16° R. [68° F.]. I obtained it by distilling common spirit of wine from half its weight of muriate of lime, dried at a nearly red heat, and drawing off only half the liquor. The product was still a little aqueous, and was farther rectified by distilling from an equal weight of muriate of lime, and again drawing off only half.

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* The French weights and measures are here retained, as they will be generally throughout this paper. Tr.

100 parts carbonic acid contain 26.14 carbon.

† Since oxigen gas does not sensibly alter its volume when converted into carbonic acid gas, the difference of weight between the two gasses in equal bulks must give the quantity of carbon contained in carbonic acid.

According to my experiments, 100 cubic inches of carbonic acid gas weigh.. 69·371 grs.

100 cubic inches of oxigen gas .... 51·237

Difference............ 18·134

Consequently 69·371 grains of carbonic acid gas contain 18·134 grains of carbon; and by the rule of proportion 69·371 : 18·134 :: 100: 26·14; so that 100 parts by weight of carbonic acid gas contain 26·14 of carbon.

In this paper I have retained the old Paris measures, to render my results more easily compared with those of others.

Three processes employed to corroborate each other.

As we cannot expect to attain the truth in a business of so much difficulty as that I had undertaken, but by coming at the same result in different ways, I employed three different processes for decomposing the alcohol.

First.

In the first I burned the alchohol by means of a lamp under a receiver filled with a mixture of oxigen gas and common air, as Lavoisier did*, and I examined the products of this combustion. The results obtained by this analysis were the least accurate.

Second.

In the second I effected the decomposition of the alcohol by the instantaneous detonation of the elastic or gaseous vapour of this liquor with oxigen gas in a Volta's eudiometer.

Third.

The third analysis was made by decomposing the alcohol in a redhot tube of porcelain.

SECT. II. *Analysis of alcohol by slow combustion in a close vessel.*

Alcohol burned slowly in a close vessel.

The lamp I employed for burning the alcohol was a graduated glass tube closed at its lower extremity. It was 6 inches high, and 3 lines in diameter internally. The wick was a slender cylinder of amianthus, passing through a metal cap, which kept it in the axis of the tube. I had ascertained by previous observation the weight of alcohol corresponding with each division of the tube, so that I could tell by simple inspection of the column of fluid in the lamp, without taking it out of the receiver to weigh it, the weight of alcohol consumed at the instant of its extinction.

This method preferable to Lavoisier's.

I preferred this method to that of Lavoisier, who weighed his lamp before and after the experiment. In this way the lamp could not be taken out of the receiver to weigh it, and analyse the air in the receiver, till the latter was cold; for it was of essential consequence to note the diminution of the volume of air by the combustion. This cooling requires near an hour; and during this period the high temperature prevailing under the receiver volatilizes a considerable quantity of alcohol, which in Lavoisier's process was confounded with the liquid that had disappeared from combustion.

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* Journal de Phisique, vol. XXXI, p. 55.

Process described.

My lamp, on the wick of which was a particle of phosphorus, was placed with a thermometer under a receiver standing in water*, and half filled with common air. To this I added oxigen gas, and the mixture occupied the space of 651 cubic inches, the barometer standing at 27 inches, and Reaumur's thermometer at 17° [70¼° F]. Before the combustion, according to analysis by Volta's eudiometer, it contained 228·25 inches of oxigen gas, and 422·75 of nitrogen.

Gasses from the combustion of 35·5 grs. of alcohol.

The lamp, kindled by a burning glass, consumed 35½ grs. of alcohol. An hour after it was extinguished, the thermometer under the receiver having fallen to 17° [70¼°], the air contained in it was reduced to 599 cubic inches; and being analysed by limewater and Volta's eudiometer it was found to consist of

Carbonic acid gas	77·87
Oxigen gas	98·42
Nitrogen gas	422·71
	599.

The carbonic acid gas in too small quantities to be absorbed.

I must remark, that the quantity of carbonic acid gas, which formed only 0·13 of the residuum, was too small to be perceptibly absorbed by the water under the receiver at the high temperature at which the process was conducted, and in the short space of time between the combustion and the examination by the eudiometer. I satisfied myself of the truth of this by direct experiment. Besides I found an advantage in substituting water for mercury under the receiver, as a small quantity of alcohol is always volatilised without being burned, even while the combustion is going on. If the receiver be lifted up immediately after the combustion, and while full of vapour, we find this diffuses an alcoholic smell. This vapour, which does not burn because it is in great part aqueous, soon condenses in the water of the trough; but if it stood over mercury, it would increase

Water preferable to mercury because it absorbs the little alcohol evaporated.

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* In Lavoisier's experiment the receiver stood over mercury.

the bulk of the air in the receiver in proportion to the alcohol it contained, even after cooling.

Calculation for the hidrogen.

When the ingenious reasoning of Lavoisier is applied to the results of this experiment, we see, that 35½ grs. of alcohol employed for their combustion 129·83 cubic inches of oxigen gas, and formed 77·87 cubic inches of carbonic acid gas. The liquid residue of the combustion of the alcohol was nearly pure water. Thus the oxigen gas I consumed, deducting the 77·87 cubic inches, that entered into the composition of the carbonic acid, was condensed by the hidrogen of the alcohol in the proportion that forms water, Consequently 129·83—77·87=51·96 cubic inches of oxigen gas must have condensed 103·92 of hidrogen gas, or double their volume.

Calculation for the oxigen.

If the weight of the carbon contained in the carbonic acid gas produced during the combustion be added to the weight of the volume of hidrogen gas just mentioned, we shall find, that the sum of these two elements amounts to little more than half the weight of the alcohol consumed. The weight deficient, or the other products of the analysis, cannot exist in the residual gas, the weight and composition of which are exactly known: it must therefore be in the liquid residue, which I have said is nearly pure water, but which I could not weigh, because it was dispersed in the apparatus. That part of the hidrogen of the alcohol, which did not combine with the oxigen added, combined therefore with the oxigen contained in the liquor itself, to form a quantity of water, which may be estimated by the deficiency in weight. On making the calculation accurately, and reducing the analysis to 100 parts of alcohol, we shall find them to contain

Carbon	36·890
Hidrogen	9·365
Oxigen and hidrogen in the proportions that form water	53.745
	100.

or,

Proportion of the elements.

or, by substituting the elements of the water,

Carbon	36·890
Hidrogen	15·814
Oxigen	47·296
	100.

A little azote likewise.

We shall find, that a small quantity of nitrogen must be included in the products of this analysis, for I found ammonia in the water formed by the combustion of alcohol. (See Sect. IV.)

Three experiments nearly agreed.

I repeated this experiment three times with nearly similar results; whence I imagine I made no mistakes, but such as arise from the process itself, which is less accurate than those I shall hereafter describe. I ought however, to compare this analysis with that of Lavoisier by the same process, except in a few minutiæ of detail.

These compared with Lavoisier's.

To reduce our results to expressions, that may be compared with each other, and freed from the different estimations we have followed with respect to the composition of water and carbonic acid gas; I must say, that, in the experiment of Lavoisier, the barometer being at 28 inches, and the thermometer at 10° [54·5° F], 10 grains of alcohol consumed 23·56 cubic inches of oxigen gas, and formed 10·194 cubic inches of carbonic acid gas; while according to mine, 10 grs. of alcohol consumed 34·111 cubic inches of oxigen gas, and formed 20·455 cubic inches of carbonic acid gas, at a similar pressure and temperature.

His alcohol weaker.

Lavoisier has not given the specific gravity of the alcohol he employed. I suppose he must have taken the alcohol considered in his time as the purest, and such as Brisson indicates in his tables, namely at a specific gravity of 0·829. This denotes a mixture of 85·63 parts of perfect alcohol, and 14·37 of water, according to the experiments of Richter, the accuracy of which I have verified. But I find, that, on deducting this proportion of water from Lavoisier's alcohol, and in other respects adopting the results of his experiment, 10 grs. of perfect alcohol would have consumed 27·518 cubic inches of oxigen gas, and formed 11·904 cubic inches of carbonic acid gas. This correction therefore still leaves a great difference between our observations.

Still the difference great.

I ought

The alcohol not changed by rectifying from muriate of lime.

I ought to remove one objection, that will no doubt be made against the kind of alcohol I analysed, it having been twice rectified from muriate of lime. Some chemists have asserted, that spirit of wine rectified from this salt acquires properties, by which it approximates to an ether. For this purpose I examined whether spirit of wine rectified by simple distillation, and without addition, would furnish by combustion results similar to those of my former analysis, with the exception of a quantity of water corresponding to that indicated by the difference of specific gravities.

The experiment repeated with alcohol at ·8248 rectified alone.

I rectified common spirit of wine by three successive distillations, without adding muriate of lime, and taking only the first product of each distillation. Thus I reduced it to the specific gravity of 0·8248, at 15° of R. [66° F.]. The process was conducted as in the former experiment. The gas in which the lamp was placed, the barometer at 27 inches, and the thermometer at 15·5° [67° F.], occupied the space of 638 cubic inches, 204 of which were oxigen gas, and 434 nitrogen. By the combustion of 33 grains of spirit of wine this was reduced to 598 cubic inches, consisting of

Carbonic acid gas	62·79
Oxigen gas	99·12
Nitrogen gas	436·09
	598.

From these results we find, that 100 parts of spirit of wine, of the specific gravity of 0·8248, contain

Proportions.

Carbon	32·24
Hidrogen	8·23
Oxigen and hidrogen, in the proportions that form water	59·53
	100.

This, allowing for the water in the alcohol, agrees with the former experiment.

Richter's table indicates, that 100 parts of spirit of wine of the density of 0·825 contain 12·8 parts of water. If from these results therefore we would deduce the composition of perfect alcohol, we must substitute 59·53—12·8=46·73 for 59.53 in the preceding analysis. This will reduce the parts representing pure alcohol to 87·2; and, making the calculation for 100 parts, they will contain

Carbon	36·97
Hidrogen	15·87
Oxigen	47·16
	100.

This proves, that the muriate of lime had not affected the alcohol.

The conformity of these results, with those of my first analysis, evidently proves, that spirit of wine rectified without addition is identical, as to its essential principles, with alcohol that has been rectified only twice from muriate of lime. Besides, the latter has none of the characteristics of ether; but retains the properties of alcohol, such as having a weak smell peculiar to spirit of wine, and not in the least ethereal. Perfect alcohol combines with water in all proportions, and its combinations with this liquid undergo changes of density nearly corresponding with those, which common spirit of wine undergoes*. It has a very small degree of expansibility, not at all approaching to that of ether the lowest rectified. Perfect alcohol forms a little soot during its combustion, but only when it is made to burn with a thick and stifled flame. Spirit of wine obtained by simple distillation likewise furnishes some under the same circumstances, but not so much, because it is less concentrated. Ether does or does not form soot according as the atmospheric air has more or less access to it during combustion. The character attempted to be derived from the presence of soot therefore, for distinguishing these two fluids, is not essential.

Possibly it may however, if too much be used, or the rectification too often repeated.

I will not assert however, that alcohol distilled a greater number of times from muriate of lime may not contain a perceptible quantity of ether: for I have observed, after having twice distilled a pound of alcohol from an equal weight of muriate of lime, that this salt, on being dissolved in water, deposited a black substance on the filter, which indicated the decomposition of a small quantity of alcohol; but this black matter was too little to be weighed, and from this result and the preceding we may conclude, that the quantity of alcohol decomposed is so small, as safely to be neglected.

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* I suppose however, that a sufficient quantity of water is first added to the perfect alcohol to reduce it to the density of spirit of wine rectified by simple distillation. Compare the changes of the specific gravity of perfect alcohol by mixture with water in *Die neueren Gegenstande der Chemie* by Richter, with the tables of Blagden and Gilpin in the Philosophical Transactions for 1790 and 1794.

(To be continued in our next.)

XV.

Letter on the Subject of the new Metals. By Mr. A. Combes.

To Mr. NICHOLSON.

Sir,

In your Journal for August is a paper by Mr. W. Cooke, of Wolverhampton, in which he states it as his opinion, that the new metal, obtained from potash by professor Davy, is not a simple body, but a compound of hidrogen, electrical fluid, and potash.

New metal supposed to be a compound.

If Mr. W. Cooke had taken the trouble to read the elaborate and refined experiments in Mr. Davy's paper (which he might have done, as it has appeared in your Journal) he certainly would never have formed so crude and unwarranted an opinion (which by the by is not original; but has been stated before by Dr, Harrington of Carlisle, in the Gentlemans' Magazine for July, except that the Doctor substitutes the word phlogiston for hidrogen). Mr. W. Cooke would have seen in Mr. Davy's paper, that water is not essential to the production of the inflammable basis of potash; and that, by burning in air, it does not produce a solution of potash, or moist potash, as it ought to do on his supposition, but pure dry solid potash.

This an unwarranted opinion, though not new.

Having criticised Mr. W. Cooke's criticism on Mr. Davy, I shall beg the liberty of criticising another communication on the same subject.

In a remark on a letter signed a "Dilletante," you say, (for it seems to come from the editor of the Journal, though from

Assertion, that the alkalis have formerly been supposed to be metallic, said to be false.

from its want of philosophical precision I suspect it has another source) that the alkalis were long ago suspected to be metallic oxides. This is not true. I have read pretty extensively in chemistry, without meeting with such a suspicion. That the alkaline earths and common earths were dephlogisticated metals, has been a very old doctrine; but I remember no such notion with respect to potash and soda. I have looked into Dr. Beddoes's Contributions; but I find no idea there of the alkalis being metallic oxides; but I have met with a much more ingenious suspicion, namely, that metals are compounds of hidrogen and azote, which, since the metallization of ammonia, does not seem so improbable.

I am, Sir, with respect,

Your obedient humble Servant,

A. COMBES.

Chelsea, Sept. 8, 1808.

REMARK.

The author of the preceding letter mistaken.

WHEN a man ventures to aſſert, that a thing "is not true," because he "has not met with it," he must have considerable confidence in the universality of his reading on the subject, the unremitting attention with which he peruses authors, and the infallible retentiveness of his memory. Admitting however, that Mr. Combes never overlooks a circumstance slightly or incidentally mentioned in a book he reads, and that his memory is too tenacious, ever to let slip what it had once received; it is surely very possible, that he might have wanted opportunity or inclination to read every work, that may have fallen into the hands of a reader much his inferior in talents; and in some of these may have been suggested hints, that have hitched in a memory far less tenacious than his. To speak with "philosophical precision" indeed, he should merely have said, that he did not recollect ever to have met with such an opinion. I can only say, that, in a book so commonly read as Fourcroy's Chemistry, the opinion, that both potash and soda are of a metallic nature is mentioned, if not directly, by implication. His words

as the opinion s mentioned by Fourcroy,

words are, in my translation of the last edition, vol. II, p. 272, art. barytes*, "the opinion relative to the pretended metallic nature of barytes, as well as of *the other salifiable*, and particularly earthy bases, will be nothing but a mere hypothesis." Now as the term *salifiable bases* is used by Fourcroy to signify the earths and alkalis; and as it cannot by any means in this passage be confined to the earths, since he immediately particularizes these, as if the opinion of their metallic nature had been more prevalent, which is undoubtedly the fact; he clearly alludes to the opinion, that potash, soda, and even ammonia were of a metallic nature. The very slight way in which he records this opinion is owing to his considering it highly improbable.

But the same opinion is given more decidedly and directly by a writer of our own country, Mr. Robert Kerr.

and is advanced by the translator of Lavoisier.

In his translation of Lavoisier's Elements of Chemistry, 2d edition, Edinburgh, 1793, p. 217, the following passage occurs in the text. "We are probably only acquainted as "yet with a part of the metallic substances existing in na"ture, as all those which have a stronger affinity to oxigen "than carbon possesses are incapable hitherto of being re"duced to the metallic state, and consequently being only "presented to our observation under the form of oxides, are "confounded with earths. It is extremely probable, that "barytes, which we have just now arranged with earths, "is in this situation; for in many experiments it exhibits "properties nearly approaching to those of metallic bodies. "It is even possible, that all the substances we call earths "may be only metallic oxides, irreducibleby any hither to "known process."

And the translator adds, p. 219, an entirely new section, sect. 6. *On the metallic nature of the earths*, in which he relates the experiments of Ruprecht and Tondi, taken from "Baron Born's description of the Cabinet of Mademoiselle "Raab;" who, as is well known, obtained metallic masses by treating barytes, magnesia, and lime severally with carbonic matter in a strong heat. This history need not be here again revived, but it is material to add, that the luminous speculations of the translator, who expressly, p. 214, mentions the alkalis as being probably metallic substances, and those of baron Born, appear to include in a general way all that the researches of Davy have realized by the skilful management of an agent, the chemical power and habitudes of which were discovered and extensively applied in this country within a few weeks after the knowledge of it was transmitted to us by Volta, one of the patriarchs of electrical knowledge and invention. It is no derogation to the merits of Davy, that he has explored the processes of nature by simplicity of investigation, and clear deductions grounded upon a knowledge of the anticedent analogies, to which he has put in no claim, and upon which it is probable he may not at present set any high value.

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* Original, vol. II. p. 196.

XVI.

Remarks on Ignition by compressed Air. In a Letter from J. A. De Luc, *Esq.*

To Mr. NICHOLSON.

SIR, *Windsor*, 15*th Oct.* 1808.

Ignition by compressed air.

I HAVE found in your No. 89, the following article: "Question respecting the *ignition* of Tinder by *compressed* "*Air*." In this question, as well as in the reply, the *ignition* is supposed an effect of the *compression* of the *air* itself; and this is the object on which I take the liberty of addressing to you some remarks.

The air not much condensed, for the piston does not recoil.

That this effect is not produced by the *compression* of *air*, is proved by some circumstances of the operation; for in fact, the *air* does not arrive to a great *density* in the instrument. If the original quantity of *air* remained sensibly in the barrel; when the piston is let free, it would recoil as much as it has been forced in, which is far from being the case. A great part therefore of that *air*, is forced out in the operation; and this even is necessary to the effect, for, if the piston did not reach almost the bottom of the syringe, the *ignition* of the tinder would not take place; and such a motion would be impossible, did all the *air*, or its greatest part, remain in the barrel.

It

The cause is the condensation of caloric;

It is not therefore, the *condensation* of *air*, which produces the *ignition*; it is the *condensation* of the immediate *cause* of *heat*; sometimes called *matter of heat*, but which, in all the records of Natural Philosophy, is named *fire*, *igneous fluid*, or their correspondents in all languages ancient and modern; and it has always been considered as an *expansible fluid*, of great power of *expansion*, when arrived to a great *density*.

as when iron is hammered red-hot.

Similar phenomenon in the air pump.

This is the cause of our phenomenon; it is produced by the same kind of operation, which brings to a *red-heat* a slip of iron very rapidly *hammered;* and that cause is the *condensation* of *fire*. That *fluid* may be *compressed* or *rarefied* in the same manner as *air*, by mechanical means. Thus in the *air pump*, which furnishes both examples at once; at the same time that the *manometer* rises or falls, by *condensing* or *rarefying* the *air* in the receiver, the *thermometer* rises or falls in it, by the *condensation* or *rarefaction* of the *free fire* mixed with the air; and both effects are produced by lessening or enlarging the *space* in which fixed quantities of the respective *fluids* are contained.

A difference in the cases from fire permeating all bodies,

whence rapidity necessary.

The only difference between the two cases proceeds from that of the permeability of bodies to these fluids. The vessels being impermeable to *air*, and made *air-tight*, the condensation or rarefaction of *air* may be produced as *slowly* as convenient, without changing the effects: whereas no vessel being *fire-tight*, the operation requires a great *rapidity*. If the same *number* of strokes of a hammer, which, by *rapidly* succeeding each other, bring a slip of iron to *incandescence*, were struck at great intervals; or if the piston which, being *rapidly* moved up to the bottom of the syringe here in view, produces the *ignition* of the tinder, is moved *slowly*; these effects are not produced: because the condensed *fire* has time to escape through the pores, in the first case of the iron, and in the latter of the barrel.

This, Sir, is what appears to me the cause of the ignition of tinder in that apparatus, which I beg you will consign in your very useful repository, if you think proper.

I am, Sir,

Your most obedient humble servant,

DE LUC.

XVII.

XVII.

On the Disadvantage of Jewelled Holes in Clockwork. In a Letter from Mr. W. WALKER, *to Mr.* J. BARRAUD.

DEAR SIR,

State of the jewelled holes of a transit clock.

I AM sorry to have delayed so long the account you wished of the state in which I twice found the jewelled holes of my transit clock, when I took it to pieces: as the vibration had each time fallen off, from being on each side the perpendicular 2° 10′ 10″, and were then no more than 1° 30′ 40″.

After 15 months going, and 2 months rest, would not go.

Oil fluid except in the jewelled holes.

Set a going again.

In 10 months again its going affected, from the foulness at the jewelling.

Excellence of its going.

In July 1805, under your direction, the clock was cleaned, and was kept regularly going till Oct. 1806, when I went from home for two months. On my return on Dec. 6th, I wound it up, but could not make it go even when I added about two pounds weight more to the clock weight. I therefore took it to pieces, and found the oil very fluid in all the holes, except those which were jewelled, where it was almost black, and very glutinous. It required great force, and some dexterity, to draw out the spindle that carries the seconds hand. I set the clock going again on the 7th of Dec., and it immediately threw out its full vibration on each side = 2° 10′ 10″; and continued to go with its usual excellence, till towards the end of Oct. 1807, when it again fell off considerably; and gained very much on its general rate. Therefore, on Nov. 23d, 1807, I again took it to pieces; found all the jewelled holes extremely foul, black, and clogged; and separated the jewels, which were strongly adhesive: yet the oil on the pallets was very fluid, and in a good state in all the brass holes. Before this cleaning the clock had gradually thrown out less and less for two months preceding, and was at this time no more than 1° 30′ 40″ on each side, but on fresh oil being applied, it immediately became = 2° 10′ 10″ on each side; and has gone with such excellence ever since, that I cannot forbear transcribing the latter part of my Journal;

nal; although in many other places, where the observations have been carefully made, I might have selected you a longer period; but the variety of this month in temperature, the thermometer in the clockcase having been at 16° and at 47°, is perhaps as severe a test as could be brought forward.

Rate of my transit clock made by Mr. Barraud.

1807.
From Nov. 26
To Dec. 9 .... + 1,3
10 .... + 1,4
14 .... + 1,3
1808.
Jan. 3 .... + 1,3
4 .... + 1,1
6 .... + 1,2
12 .... + 1,2

These were the only days on which I could get an observation.

I remain, dear Sir,
Your obliged Friend,
And humble Servant,
W. WALKER.

Manor House, Hayes, Middlesex,
20th January, 1808.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

At the last meeting of the Wernerian Natural History Society, (1st August) Dr. James Ogilby of Dublin read a very interesting account of the Mineralogy of East Lothian, which appeared to have been drawn up from a series of observations made with great skill, and was illustrated by a suite

Mineralogy of East Lothian.

suite of 350 specimens laid upon the table.—As the county is in general deeply covered with soil, and profusely clothed with vegetables, the determination of the different formations must have been a work of considerable labour; and the skill, judgment, and perseverance of the observer, must have been frequently put to the trial. The doctor, after describing the physiognomy or external aspect of the county, gave a particular account of the different formations of which it is composed. They are as follows:—transition, independent coal, newest floetztrap, and alluvial. When describing the different transition rocks, he alluded particularly to the supposed granite of Fassnett, (described by Professor Playfair in his Illustrations of the Huttonian Theory*), which he proved to be a stratified bed of transition greenstone. The description of the rocks of the newest floetz-trap formation was particularly interesting, not only on account of the beautiful transitions he pointed out, but also as it proved the existence of a considerable tract of these rocks in Scotland, where their occurrence had been disputed. He enumerated and described the following members of this formation:—traptuff, amygdaloid, clay-stone, basalt, porphyry slate, and porphyry slate inclining to greenstone. He found the traptuff, which is a coarse mechanical deposit, forming the lowest member of the series, and resting immediately on the coal formation: on this tuff rests amygdaloid containing fragments: above this amygdaloid is common amygdaloid free of fragments; this, in its turn, is covered with basalt: the basalt gradually passes into and is covered with porphyry slate: and the porphyry slate, in some instances, appears to pass into greenstone, which forms the uppermost portion of the formation:—so that we have thus a beautiful series of transitions from the coarse mechanical, to the fine chemical; that is, from traptuff to porphyry slate inclining to greenstone. The doctor also remarked, that the amygdaloid contains crystals of feldspar which have an earthy aspect; the basalt, crystals of feldspar possessing the characters of common feldspar; and the porphyry slate, glassy feldspar;—facts which coincide with, and are illustrative of the increasing fineness of the solution, from the

Newest floetz-trap formation.

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* Page 328.

oldest to the newest members of the formation. In the course of his paper, the doctor gave distinct and satisfactory answers to the following queries, which had been proposed by Professor Jameson: 1. Does the Bass Rock in the Frith of Forth belong to the newest floetztrap formation? 2. Does the sienitic greenstone of Fassnett in East Lothian belong to the transition rocks, or to the newest floetztrap formation? Are the geognostic relations of the porphyry slate, or clinkstone porphyry, of East Lothian, the same as in other countries? The doctor announced his intention of reading, at the next meeting of the Society, a description of the different veins that occur in East Lothian, and of giving a short statement of the geognostical and economical inferences to be deduced from the appearances which he has investigated with so much care. It is indeed only by investigations like those of Dr. Ogilby, that we obtain any certainty respecting the mineral treasures of a country; and such alone can afford us data for a legitimate theory of the formation of the globe.

New species of fasciola occasioning a disease in poultry.

At the same meeting, a communication from Col. Montague was read, describing a new species of fasciola, of a red colour, and about an inch long, which sometimes lodges in the trachea of chickens, and which the colonel found to be the occasion of the distemper called the *gapes*, so fatal to these useful tenants of the poultry yard. The knowledge of the true cause of this malady will, it is hoped, soon be followed by the discovery of a specific cure: in the mean time, a very simple popular remedy is employed in Devonshire: the meat of the chicks (barley or oat meal) is merely mixed up with urine, in place of water, and this prescription is very generally attended with the best effects.

To CORRESPONDENTS.

Mr. Gough's answer to Mr. Barlow, and the communication from Mr. Moore, in our next.

O's letters will be attended to.

METEOROLOGICAL JOURNAL

For OCTOBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Maker,
in the STRAND, LONDON.

SEPT.	THERMOMETER.				BAROMETER,	WEATHER.	
Day of	9 A. M.	9 P. M.	Highest.	Lowest.	9 A. M.	Night.	Day.
26	54	60	61	48	30·09	Cloudy	Fair
27	50	47	56	42	30·01	Fair	Ditto
28	44	43	50	40	29·68	Cloudy	Ditto
29	43	43	54	38	29·44	Fair	Rain
30	42	45	55	40	29·58	Ditto	Ditto
OCT.							
1	42	44	52	39	29·90	Ditto	Fair
2	45	52	54	45	29·76	Cloudy	Rain
3	50	49	54	46	30·02	Fair	Ditto
4	51	52	55	47	30·25	Ditto	Ditto
5	52	52	60	46	30·18	Ditto	Fair
6	51	56	58	51	30·10	Ditto	Ditto
7	54	52	54	45	30·09	Cloudy*	Ditto
8	48	46	51	40	29·34	Rain	Rain
9	46	42	53	42	29·78	Fair	Fair
10	52	49	55	42	29·78	Ditto	Ditto
11	46	53	56	44	30·04	Rain	Rain
12	48	50	52	37	29·97	Cloudy	Ditto
13	40	44	53	41	30·10	Ditto	Fair
14	47	45	52	40	29·50	Ditto	Rain
15	43	46	48	41	29·21	Fair	Ditto
16	44	44	49	40	29·60	Ditto	Ditto
17	45	41	48	40	29·59	Ditto	Fair
18	42	47	50	39	29·75	Rain	Ditto
19	42	46	50	42	29·52	Fair	Rain
20	42	46	51	42	29·70	Cloudy	Ditto
21	47	42	51	41	29·34	Fair	Ditto
22	44	41	49	35	29·49	Ditto	Fair
23	40	48	50	47	29 82	Rain	Rain
24	46	43	50	38	29·38	Fair	Ditto
25	43	50	52	47	29·69	Rain	Ditto

* High wind, hard rain at midnight

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1808.

ARTICLE I.

Answer to Mr. Barlow's *Remarks on the Essay on Polygonal Numbers. By* J. Gough, *Esq.*

To Mr. NICHOLSON.

SIR,

MY answer to Mr. Barlow's criticism on the solution of Fermat's theorem was in the possession of that gentleman, I believe, prior to the date of his letter inserted in your Journal, Vol. XXI, p. 118. As Mr. Barlow does not think proper to make use of my permission to publish the reply, I am under the necessity of repeating in your present number arguments, which have been already stated in a private correspondence.

Answer to Mr. Barlow's criticism.

Mr. B. opens his criticism by admitting the first three propositions, with their corollaries, to be correct; but he does not see in what manner they are to be applied to the general demonstration. This objection may be answered thus: If the remaining propositions be derived from these three, or any one of them, the necessity of inserting them all is established, because the third is derived from the second, and the use of the first appears in the course of the essay.

1st objection answered.

essay. That the propositions following the third are derived from those which precede them is a fact, that is proved by the references; consequently, if my paper contain a general demonstration of the theorem proposed, the necessity of the first three propositions is proved.

2d objection answered.

Mr. Barlow's second objection charges me with false logic: and this gentleman states a sophism, which he considers to be similar to the argument used in cor. 2, prop. 4, of the essay on polygonal numbers. He observes, "that "the author of this essay might, with as much propriety, "have said, that every natural number is either even or "odd, and every aggregate of polygonals being also either "even or odd, therefore every natural number is the aggregate of polygonals." Mr. B. rests his refutation of the argument used in cor. 2, prop. 4, on the supposed similarity of it and the preceding sophism; if then I can show these two to be dissimilar, his second objection must be pronounced futile. To do this, I may observe, that numbers, like most other things, are aggregates of qualities, not single qualities, otherwise there could be no more numbers than qualities; that is, a number, beside being odd or even, is prime or composite, rational or irrational. This consideration shows the nature of the intended fallacy contained in the preceding sophism; for it maintains two aggregates of qualities to be the same; because they have one of these qualities in common. This I presume is an objection, to which the demonstration in question is not liable: for equality constitutes identity in numbers; that is, if any one of two or more equal numbers possess any three of the qualities pointed out above, or any of the properties contained in the definitions to the 7th Book of the Elements, all the rest of them possess just the same, neither more nor less (by axiom 1st of the same book). Now it is shown in the first corollary to the 4th proposition, that every aggregate of polygons of the denomination m is of the form $p + \overline{m - 2} . s$; where p is limited by 0 and $m - 3$; and s is indefinite: hence it follows, that each aggregate of such polygons is equal to an assignable value of $p + \overline{m - 2} . s$. Moreover it appears from the second corollary to the same proposition, that every natural number is of the form $p+\overline{m-2}.s$, limited as above; where s may be found, the number being given with m and p; but this value of s substituted in the form $p+\overline{m-2}.s$, gives an aggregate of polygons of the denomination m, which is true in all cases; it is therefore a universal truth not admitting of one exception. The preceding facts appear to give indisputable accuracy to the following syllogism: every natural number is equal to an assignable value of the form $p+\overline{m-2}.s$; and there is an aggregate of polygons of the denomination m equal to the same value of the same form; therefore every natural number is an aggregate of such polygons; because things, which are equal to the same thing, are equal to one another; Euclid, Axiom 1, Book 1; and equal numbers have been shown to have the same qualities neither more nor less. The supposed similarity betwixt my critic's sophism and the preceding mode of argument appears to be done away; for he proceeds on the supposition, that the sameness of one quality constitutes identity in numbers; but the first axiom of the 7th book of Euclid is the foundation of my reasoning; namely, that a perfect agreement in qualities produces the same thing, namely, identity of numbers. My opponent, in fact, does not rely altogether on the similarity of his intended, and my accidental sophistry; for he produces a second sophism, and pronounces it to be strictly analogous to mine, though it differs in every particular from his former parody of my supposed mistake. Mr. B. observes, that "every natural number is of the form "$p+\overline{m-2}\times s$; and, every square number being also of "the form $p+\overline{m-2}.s$, therefore every natural number is "a square number." It is true, that every square number is of the form $p+\overline{m-2}.s$; but then s is limited, being of the form $s=\frac{q^2+2qv+v^2-p}{m-2}$, where $q^2=p$ or the next greater square when p is not a square, and v is to be taken so as to make s a whole number; but s is unlimited in the case of natural numbers; therefore, by the rules of logic, every square integer may be proved to be a natural number, but not every natural number a square integer. On the contrary, s is unlimited in the aggregates of polygons, as well as in natural numbers; therefore, my opponent's second parody is equally unsuccessful with his first; because its imaginary resemblance to my syllogism has been shown to be spurious. In the conclusion of this objection it is remarked, that the corollary under consideration might be assumed as a postulate, i. e. as a self evident problem; but far from treating it either as postulate or axiom, he agrees with me in giving it the importance of a theorem, and demonstrates it accordingly.

3d objection answered.

Mr. Barlow's third objection is occasioned by an obvious mistake on his part: I have said, that, if $e = y + t$ can be resolved into $m - f$ polygons, $e + f$ may be resolved into m polygons of the same denomination. My critic puts a construction on this expression, which makes me suppose, that $y + t$ can be resolved into $m - f$ polygons of the denomination m, in all cases. This is evidently a misconception; for, had my opinion agreed with Mr. Barlow's interpretation of it, why have I attempted to demonstrate the theorem of Fermat; the truth of which I am supposed to assert without demonstration in the preceding quotation from my essay? The genuine meaning of the passage is obviously this; if $e = y + t$ can be resolved into $m - f$ polygons in any one case; $e + f$ may be resolved into m such polygons in the same case; which construction of the expression refutes this part of the criticism.

The proposition shown to be universal.

All my opponent's objections have now been controverted; but he farther remarks, that there is a difference betwixt doing a thing, and proving that it may be done in all cases. The justice of this observation obliges me to show, that my essay also contains the principles of the latter demonstration. For this purpose let the reader look at example 2, prop. 7, which will assist him in the following reasoning. If $e = y + m - 1$, it may be resolved into m polygons (by cor. prop. 6): again, if $e = y + m$, it may be resolved into two polygons, which are less than m; here $f = m - 2$, and $e + f = y + 2m - 2$ is resolved into m polygons, (cor. prop. 6): moreover, if $e = y + 2m$, it consists of three polygons (by prop. 6, and cor. 1, prop. 2), but three is the least value of m; hence all the numbers from y to $y + 2m$ are properly resolved, except $y + 2m - 1$. Now, let a be the index, which resolves $y + 2m - 2$ into polygons; and the same, a, will resolve $y + 2m - 1$ into $m + 1$ polygons; but the next value of $a = a + 2 - m$ (cor. 1, prop. 4); which will resolve $y + 2m - 1$ into m polygons, or less, (cor. 2, prop. 5). In general, if $e = y + t$ can be resolved into m polygons by the index a, the next index $a + 2 - m$, will resolve $e + 1$ into m polygons, or less (cor. 2, prop. 5).

JOHN GOUGH.

Middleshaw, October 15, 1808.

II.

Description and Use of a Sea Log, and Sounding Machine, invented by Mr. EDWARD MASSEY, *of Hanley, in Staffordshire.*

An accurate log wanted.

TO the nautical reader the advantages resulting from a log, that will give a dead-reckoning free from errour, or nearly so, must be sufficiently obvious; and to others it would be superfluous to point them out. The principle, on which Mr. Massey's patent log is constructed, is not new; but every application of it to practice has been found defective, and this is the difficulty the patentee has had to surmount. To understand the manner in which it acts, see Pl. 7, where *a*, fig. 1, is that part of the log which registers the distance sailed, and is therefore called the register; it contains within itself a set of wheel work, which operates upon the fingers of the several indices, 1, 2, and 3. *b* is the rotator, a hollow cylinder, made air-tight, and so nearly of the same specific gravity as water, as to float when drawn forwards with the velocity of mere steerage way. On this rotator are fixed four vanes placed obliquely. It is then fastened to the register by a cord, *c*, about six feet long*: to the loop-hole

New log described.

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* This cord is shown scarcely one tenth part of its proper length in the engraving: it would have been an unnecessary extension of the plate to represent it otherwise, as it may so readily be conceived.

at the other end of the register is secured another line, *e*, of sufficient length to extend beyond the eddy of the vessel's wake.

The finger on the index 1 revolves once while the log moves forward one mile; that on the index 2 moves once round in going ten miles; that on the index 3 makes one revolution when the distance sailed is one hundred miles. When the machine is to be used, all the fingers of the indices are set to 0, and both register and rotator committed to the water.

Its mode of action.

As the vessel moves forward, the log must follow, and from the obliquity of the vanes it is evident the rotator, *b*, must revolve quicker or slower, correspondent to the ship's velocity. This rotatory motion is communicated by the cord *c* to the universal joint *d*, connected with the wheels, which consequently revolve with the rotator and cord, and thus the actual space passed through, in any given time, is registered on the indices.

Registers the whole distance sailed.

Every occasional or momentary acceleration or retardation of the vessel, from irregularity of wind, or other causes, which are either altogether passed over, or very vaguely guessed at, in general, are accurately registered on this machine, which not only gives the actual rate of sailing, but the actual distance sailed, since the last inspection.

A very little reflection will convince any observer of the great superiority of this machine over all others which have been hitherto introduced.

Former attempts have failed,

It may appear rather presumptuous to criticise the labours of Smeaton, and many others, whose endeavours were not crowned with complete success: but it is necessary to point out where their plans failed, in order to prove the very superior advantages of Massey's log; for though some of the machines answered their purposes tolerably well under certain circumstances, none of them were nearly correct under all circumstances. Some were erroneous when the ship moved less than four miles in the hour, and others became so when the rate was increased.

and why.

In most of the former inventions, the first mover was a spiral, or a rotator in the shape of a Y, and was generally attached to a register kept in the ship; and as it was absolutely necessary, that this first mover should be out of the wake of the vessel, it had a length of fifty yards of cord, or more, to carry round with it every time it revolved. The friction caused by this operation was such as to preclude all hopes of accuracy; it was useless in an agitated sea, the rope was very liable to kink, and in fast sailing the rotator would sometimes fly out of the water*. These circumstances rendered it impossible, that the rotator should make the same number of revolutions in passing through a given space, under different velocities; and hence inaccuracy was unavoidable. To get rid of this friction of the long line, the rotator has, in some instances, been enclosed in a cylinder, and a register been attached to the outside. But though the defect of excessive friction was thus surmounted, still greater inconveniences resulted. It may be sufficient to mention, that the cylinder, not presenting itself horizontally in the water, was liable to alter its position whenever the velocity of the vessel was changed, which caused an eddy, or dead water, to remain in the cylinder; and, of course, the rotator was liable to err, in proportion as the cylinder lost its horizontal position.

After thus hinting at the imperfections of other previous methods of constructing logs, it remains to point out wherein Massey's plan differs.

Difference of the present log.

Friction trifling

Friction is the principal cause of mechanical theories varying so widely from actual experiment. In some machines one third is allowed for its effect, while the operation in others is nearly suspended, and what appeared very plausible in theory, is found totally useless in practice. Thus the friction on a rope long enough to extend beyond the eddy of the vessel's wake would, in many circumstances, on the old plan, totally impede the action of the rotator†. Under this impression, the friction in Massey's patent log is reduced to almost nothing by the following simple contrivance. The whole log, consisting of the register, *a*, connecting cord, *c*, and the rotator, *b*, is committed to the water, by a log line of sufficient length to reach beyond the eddy of the vessel's wake. As the ship moves forwards, the rotator, and cord, *c*, between it and the register, revolve and set the wheels into motion; nor has the roughest sea been found to prevent this action.

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* Smeaton, in the account of his experiments on Saumarez's log, in the Philosophical Transactions, observes on this subject "Upon making up the account of this run, I found the number of rotations were "less by one full third than they ought to have been, compared with "the former observations, which afforded me a convincing proof, that "this instrument was considerably retarded in quick motions"

† Smeaton, in the account of his experiments in the work before quoted observes: "During this run, I observed that the resistance of "the water to the line and plate was very considerable, and increased the "friction

Rotator always horizontal,

The rotator also, in this log, is so constructed as always to preserve a horizontal position, by being made nearly of the same specific gravity as water; which is effected by means of an air tube passing through its centre: an indispensible requisite, which no former machine possessed; and for the want of which, they could not preserve that horizontal position in fast and slow sailing, which is absolutely necessary to obtain any true result.

and accurately adjusted.

Another very important improvement consists in the contrivance for regulating the rotator, by which means every rotator revolves once on its axis in passing the same space: as it was found utterly impossible to construct two rotators so exactly alike as stated by Smeaton, without means of adjustment.

General properties of this log.

To sum up the properties of Massey's patent log, in a few words it may be observed,

1. It will give the true distance sailed, from steerage way, to any velocity with which the swiftest sailing vessel can move.

2. It not only gives more accurately than the common log the rate of sailing, but the actual space sailed through since the last inspection.

3. It is attended with less trouble than the common log, and no mistakes can possibly arise from the result it gives.

Another advantage it has in giving us a

It remains to point out one great and desirable advantage, which may very reasonably be expected to result from the use of this log, and that is, a more complete knowledge of the

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"friction of the spindle so much, as to prevent it from beginning to "turn, till the plate had twisted the line to such a degree, that when it "did set a going it would frequently run one hundred and fifty or two "hundred turns at once."

the currents in various parts of the ocean, which has hitherto been very imperfectly attained; as it was not possible to know, with any certainty, whether the wide difference found between the real distance, and that given by the common log, was caused by the known imperfections of that method of reckoning, or by the operation of currents.

knowledge of currents.

Remarks on this by Dr. Maskelyne

Dr. Maskelyne, in the same work just quoted, further observes: "There is another argument which adds much "strength to the foregoing ones, and greatly enforces a "uniform and correct length of the logline, on board all "ships; that in many parts of the ocean, especially be"tween the tropics, and near most head-lands, there are "considerable currents, which must introduce a fresh er"rour into the reckoning; and if this errour should happen "to combine with that already produced by a wrong length "of the logline, as it may as well as not, it is not easy to "say how far the total errour of the reckoning might go, "or to what inconveniencies or dangers the ship might be "exposed on that account. But if the just and proper "length of the logline were used on board of all ships, "they would be then liable only to the errours of the cur"rents themselves; and even these, as far as they are con"stant and regular, might be found out and ascertained, "from the journals of several ships, which would then agree "much nearer with one another." And Smeaton observes, "that it is for want of a means of measuring the way of a "ship through the water, (and this compared with other "check observations,) that the drift and velocities of the "principal currents have not already been determined."

and Mr. Smeaton.

But admitting the common logline and glass were perfectly uniform in each ship of a fleet, yet the result would still be too erroneous to expect this very desirable knowledge of the currents to be derived from a comparison of the several journals. Massey's patent log holds out, however, more than a probability of effecting this important end. It appears by a letter from Captain Whittle, of the Lord Nelson, that he found the distance run from the island of Ila, to St. John's harbour, Newfoundland, by Massey's log, to agree with the known latitudes and longitudes of both places, within eight miles. Now had he sailed in company with several

Reckoning to Newfoundland kept time to eight miles.

veral other ships, supplied with the same log, which had kept tolerably well together during the whole voyage, and it had been found (which is more than probable) that all their reckonings corresponded with his; the difference between the true distance, and the distance given by the log, might with the greatest propriety be ascribed to the operation of currents; the existence of which would consequently be discovered, as far as related to those seas.

From the common method of taking soundings many ships lost.

The importance of obtaining true soundings at sea must be admitted by every seaman; and it is rather singular, that no other method than the common lead has hitherto been brought into use; as its imperfections are very generally acknowledged.

Causes of its inaccuracies.

Many vessels have been lost, by depending upon the soundings taken in the usual way. The difficulty of obtaining the true perpendicular, and the uncertainty as to the exact moment when the lead strikes the bottom, upon which the accuracy of the result depends, must always prevent the possibility of obtaining the true depth, while the ship has any considerable way upon her. Indeed, it has been acknowledged by experienced seamen, during some experiments, made at various times, in the river Mersey, that they could not depend upon the common lead, when going five or six knots in the hour, in ten or twelve fathoms of water. When the depth is considerable, the vessel must be hove to, which is an operation attended with great loss of time, and sometimes considerable injury to the sails; and during a chase, this inconvenience must be particularly felt.

New method.

Massey's sounding machine is as great an improvement upon the common lead, as his patent log is upon the common log. A rotator on the same principle as that to the log registers the perpendicular descent of the lead, without any respect to the length of line paid out, which, in the usual method of taking soundings, is the chief guide to the mariner in judging of the perpendicular depth, and is apt to deceive him much.

Soundings taken in 30 fathoms without heaving to.

True soundings may be taken with this machine in thirty fathoms water, without the trouble of heaving the vessel to, although she may be going at the rate of six miles in the hour. True soundings may also thus be obtained in very deep water, where it is not possible to take them by the common lead.

Principle of the machine.

This sounding machine is on the same principle as the log, for it is evident, that, if the end *e* of the register, *a*, (*fig* 1) were projected into the water, and suffered to descend, the rotator would follow, and register the exact depth, as well in a perpendicular, as in a horizontal position.

But though the principle of the two machines is the same, their construction necessarily differs considerably, as will be perceived on reference to the plate.

Description of it.

Fig. 2 represents the sounding machine. *a* is the sounding weight, containing a register, 1, 2, with two dials: the hand of the dial 1 makes one revolution when the weight has descended twenty fathoms, the other revolves once when the descent amounts to five hundred fathoms. A rotator, *b*, similar to that attached to the log, communicates with the wheel work of the dials 1, 2, by means of the rod *c*, on which there are three universal joints, 3, 4, and 5. This rod is supported during the descent of the weight, by the drop, *d*, at the end of which is a fork, 6, and a friction wheel, 7.

Method of using it.

When the machine is to be used, a sounding line is fastened to the ring, *e*; and one of the vanes of the rotator is slipped into the spring 8: the rotator will then be in the position indicated by the dotted lines, *x*. The indices must be set at 0, and the cover or lid, *f*, be shut. The machine must then be projected perpendicularly into the sea. As soon as it reaches the surface, the resistance of the water forces the dotted rotator, *x*, out of the spring 8, and it assumes its perpendicular direction as represented by the rotator *b*. As the machine descends, it is evident the rotator will revolve, and its motion be communicated freely past the friction wheel 7, and the universal joint 5, to the wheel work of the dials 1, 2, and thus indicate the space passed through in fathoms. When the machine has arrived at the bottom, the rotator, as it is no longer buoyed up by the reaction of the water, will fall to the bottom, quitting the fork of the drop *d*, which will also fall from its horizontal position, and in its descent, by means of the locking rod 9, prevent the rotator from revolving as the machine is drawn up. When at the bottom, the rotator will be in the position of the dotted lines *y*.

No mistake can arise from it.

This machine, simple in its construction, and scarcely more liable to accident than the common lead, ascertains, with the utmost precision, the perpendicular depth, by the mere act of descent through the water. No mistake can arise from that common source of errour, the drift or lee-way of the ship during the time of descent; nor does an operation of such importance depend upon the uncertain sensation caused by the lead striking the bottom, on which the accuracy of the common log altogether depends, and which, it is well known, frequently and materially misleads the best seaman: for though a thousand fathoms of line were paid out, in the smallest depth of water, no inaccuracy could arise, as the perpendicular depth, at the point of heaving, would be registered on the index. The only inconvenience experienced would be the additional labour necessary for hauling in the excess of line. The most inexperienced person may use this machine, without risk of errour, in the most turbulent sea, and during the night.

The advantages already enumerated would render the sounding machine of great importance; but there are other properties of still more consequence.

Farther advantages.

To heave a ship to, in order to obtain soundings, on a lee shore, in stormy weather, is a very disagreeable operation, attended with much trouble, and loss of way; also with considerable danger to the ship's sails; indeed, it would often, under such circumstances, be attended with great hazard to the safety of the ship. To avoid these unpleasant consequences, the master sometimes adopts a measure, which he conceives to be the less exceptionable alternative, by running on without sounding at all.

Sounding in 60 or 80 fathoms while going 3 knots an hour.

To prove how much inconvenience and danger are avoided by Massey's lead, it is enough to state, that soundings may be taken in depth from 60 to 80 fathoms, while the ship is under way, at the rate of three miles an hour; and as the rate of sailing may be still materially reduced, without entirely stopping the vessel, or altering her course, so may soundings be had, to any depth required, while she is under way.

Its superiority exemplified.

In order more clearly to show the superiority of this machine, and make it apparent, that the quantity of stray-line veered out does not at all affect the truth of the result: suppose the common lead thrown from the mizen chains of the ship, which may be represented by the point *a* of the triangle *a b c*, (fig. 3), and that the ship has moved forwards through the space equal to the line *b c*, while the lead has descended through the line *a c*; it is evident, that it is impossible, in this case, to ascertain the exact depth, as a quantity of line, equal to *a b*, would be paid out, whereas the true depth is equal only to the line *a c*, which is much less. But the case is very different when the patent sounding machine is used, as the operation ceases when it has reached the bottom; nor is the stray-line, *a b*, whatever its length, at all taken into the account.

Takes accurate soundings at any depths.

It has been found extremely difficult, and sometimes impossible, to obtain soundings in very deep water with the common lead, which may perhaps be thus accounted for. The common line which is used for sounding, though, if left to itself, it would sink in water, yet its descent would be much slower than that of the lead, separately; it consequently follows, that the lead must be so much impeded by carrying the line with it, that when it does reach the bottom, there will be scarcely any sensible check to enable the seaman to know the precise moment. Indeed, if he can ascertain even this to a certainty, he still cannot depend upon the truth of his soundings; for if there be the least drift or current, the line itself will assume a curve, similar to that of the line of a kite in the air. These two causes will always operate against the perfection of the common mode of sounding.

After so fully describing the principle of the patent sounding machine, it is scarcely necessary to prove, that it is liable to neither of the foregoing objections; and it may be sufficient to say, that, as it will certainly find its way to the bottom, if a sufficient portion of stray-line be allowed to guard against its being checked in its progress, and the certainty of its having reached the bottom may be ascertained by the arming, there can be no doubt of the practicability of its obtaining soundings, in any depth, and no reasonable doubt of their correctness when obtained.

The rotator does not impede its descent,

From the construction of this machine, it might be imagined, that the rotator would impede its motion through the water, and that it could not descend so rapidly as the common lead; but during repeated trials, in thirteen fathoms water, in which the rotator was frequently detached, and the lead suffered to descend alone, there was no difference perceptible in the time of their descent, though an excellent quarter-second stop watch was used during the experiment, to detect any change. The following table shows how very uniformly the times of descent corresponded with the depths in fathoms, during a series of trials made on the river Mersey, with the patent lead, weighing fourteen pounds.

as shown by experiments.

The manner of conducting these experiments was such as is deserving of perfect reliance. Two pilots, of well-known ability and experience, were employed: one threw the lead, and the other, the moment he found, by the slackening of the rope, that the weight had arrived at the bottom, cried, 'stop,' to a third person who held the watch.

Time of descent.	Fathoms.	Time of descent.	Fathoms.
2 seconds	2½	7¼ seconds	11½
2½ ———	3	7¼ ———	11½
3 ———	4	7¼ ———	11½
5 ———	8	7½ ———	12
5½ ———	8½	7¼ ———	12¼
6 ———	10	8 ———	13
6 ———	10	8¼ ———	13½
7 ———	11¼	6 ———	10

Taken when under sail, at upwards of five knots in the hour.

Several captains and masters in the navy have made trial of the log and sounding machine, and given very favourable reports of their performance. Of these the two following may be selected as specimens.

San Joſef, 12*th Dec.* 1806.

Testimony of the performance of the sounding machine,

Having several times, and in different depths and rates of sailing, tried Mr. Edward Massey's patent sounding machine, which is, in my opinion, a most excellent invention, as correct soundings were gained in fifty-five fathoms, with a strong breeze, going six knots, by only passing the lead to the quarter-boat, attaching a hand lead about thirty fathoms from the machine, (which I think, is in such cases necessary :) and about ninety fathoms of line out: at another trial, to compare the old with the new method, going five knots and a half, correct soundings were ascertained by the machine in fifty-two fathoms, by passing the line to the main-chains, when we could barely get the depth in the old way, by carrying the lead to the spritsail-yard, notwithstanding the immense length of a firſt rate, and daylight in our favour; and not even then, if we had not had knowledge of the depth nearly, that being a check or caution not to give too much line off the reel, there being no time to gather in the slack, which would be the case were we sounding in an unknown place, by the old method. The invention is the more valuable, as the process is the most simple, the whole being understood, by seeing it once in use.

I therefore consider it a valuable improvement in navigation; as in frequent, and various cases, soundings could not be gained without it. The advantages are many, such as in chase, or being chased; on a lee-shore, or doubtful of it; and to save time in running for the desired port*.

R. J. NEVE, Captain.

N. B. It will be necessary in the practice of the new method of sounding, to have line of different sizes, in proportion to the depth of water; as by the ship passing at the rate of eighl or ten knots, it will require the best of lines to haul in the lead, and should be made of a much superior quality to those at present supplied to the navy.

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* The honourable Navy Board have adopted the sounding machine for the use of his majesty's navy, and have favoured the inventor with an order for five hundred machines.

H. M. S. San Josef, in Torbay, 12th Dec. 1806.

SIR,

and of the log. In obedience to your orders, we have been particular in attending to the use of Mr. Edward Massey's Patent Log, and from every opportunity that offered during our cruize we are strictly of opinion, that it has the merit of accomplishing the end for which it is intended.

On some trials made with it, and the common log, they perfectly agreed, at other times they differed a little, but last night bearing up for Torbay, with a run of eighty miles in squally weather, there was a difference of nine miles: but agreeably to our reckoning the patent log was perfectly correct; we therefore consider it an important improvement in navigation, and the more so, as the instrument is simple and easy to be generally understood.

The chief things necessary to be observed are to secure the tow-line as near the surface as possible, to prevent the machine from quitting the water in an agitated sea, and fast sailing, and not to be less than sixty fathoms long in a first rate, to prevent it from being affected by the eddy of the ship's wake.

We are, Sir,
Your most obedient humble servants,
R. J. NEVE, Captain.
THOMAS MOORE, Master.

To sir Charles Cotton, bart., viceadmiral of the red, &c.

III.

Observations on the Problem respecting the Radius of Curvature. In a Letter from W. Moore, *Esq.*

To Mr. NICHOLSON.

SIR,

IF the following observations on the problem respecting the radius of curvature, should be found to deserve a place in your Philosophical Journal, the insertion of them will greatly oblige,

Sir,
Your most obedient humble servant,
W. MOORE.

An attempt to show, that the nature of the problem respecting the radius of curvature does not involve in it the consideration of second fluxions; but that they are made to enter into the definitive expression as a matter of mere convenience.

Problem respecting the radius of curvature does not involve second fluxions.

Definition. If one end, O, of the thread A L D O, Pl. vii, fig. 4, be first fixed to the point O, in a curve L D O, concave the same way, and afterward the thread be put about the said curve so as to touch it in every part: then if the other end, A, of the thread be tightly moved in the same plane with the curve L D O, the said end, A, will describe a curve A B I, called the *involute* curve to L D O which is the *evolute:* and the right lines D B, O I, are said to be radii of curvature.

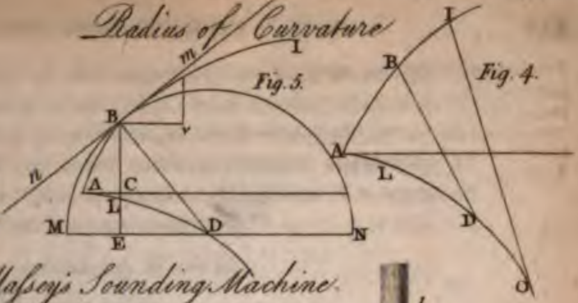
It is evident from the method of generation of the curve A B I, that if at any point D, in the *evolute* L D O, the string should cease to unwind itself and the radius DB continue to revolve about D, as a centre (see figure 5), the circle thereby described would have the same degree of curvature as the *involute* at the point B; and that a tangent drawn to either curve would be common to both. Moreover, because the said two curves, viz. the *involute and circle,* have the same curvature at the point B, and their continuations one and the same curve, namely, the circle where radius is D B; the fluxions of the absciss and ordinate of the one, will be equal to the fluxions of the absciss and ordinate of the other, and consequently the same will hold of any other order of fluxions whatever. This being premised, let it be required to find a definitive expression for the radius of curvature of any curve, as A B I. For which purpose let as usual the absciss and ordinate A C, B C, of the curve A B I, be denoted by x and y; also A B=Z, and put B E the corresponding ordinate of the circle M B N=*v*. Then the triangles B*vm*, B E D, being similar, we have B*v* : B*m* :: BE : BD; or $\dot{x} : \dot{z} :: v : \mathrm{BD} = \frac{v\dot{z}}{\dot{x}}$. Now this expression is general; but being in terms of the ordinate of the circle M B N and unequal to B C, the ordinate of the circle; it cannot with convenience be applied to curves whose equations are generally expressed in terms of their

Problem respecting the radius of curvature does not involve second fluxions.

own ordinates: we must therefore in order to adapt it to practical purposes find a value for v, in such terms as shall be consonant to the characters in which the equations of curves are generally written, viz. those expressing the absciss and ordinate: in order to which, let the above expression be put again into fluxions, and made equal to nothing (it being a constant quantity) and we shall have $\frac{\dot v\dot z+v\ddot z}{\dot x}=\frac{v\ddot x\dot z}{\dot x^3}=o$; and $v=\frac{-\dot v\dot z\dot x^2}{\dot x^2\ddot z-\ddot x\dot z}=\frac{-\dot y\dot z\dot x^2}{\dot x^2\ddot z-\ddot x\dot z}$; therefore, B D $=\frac{v\dot z}{\dot x}=\frac{\dot y\dot x\dot z^2}{\dot x^2\ddot z-\ddot x\dot z}$, another general expression in terms of the fluxions of the arc, absciss and ordinate of the involute—but this, like the other is still inconvenient for practice; yet the difficulty may be removed very easily by expunging $\ddot z$: for put $\dot x^2+\dot y^2=\dot z^2$ into fluxions, and we get $\dot x\ddot x+\dot y\ddot y=\dot z\ddot z$; and $\ddot z=\frac{\dot x\ddot x+\dot y\ddot y}{\dot z}$ so that our last expression becomes $\mathrm{BD}=\frac{-\dot y\dot x\dot z^2}{\dot x^2\ddot z-\ddot x\dot z}=\frac{-\dot y\dot x\dot z^3}{\dot x^3\ddot x+\dot y\ddot y\dot x^2-\ddot x\dot z^2}=\frac{-\dot y\dot x.\ \overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{\dot x^3\ddot x+\dot y\ddot y\dot x^2-\ddot x\dot z^2}$; the general value for the radius of curvature, when both the absciss and ordinate flow inconstantly: but as all curves may be generated, either by the uniform increase of the absciss and inconstant variation of the ordinate; or by the uniform flow of the ordinate and variable flux of the absciss; we are at liberty to assume the first fluxion of either constant as it may suit our convenience; and thus simplifying the expression, avoid the trouble which would otherwise arise.—Thus, if $\dot x$ be supposed constant, the expression will be $\frac{\overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{-\dot x\ddot y}$; and if $\dot y$ be made constant it becomes, $\frac{-\dot y\dot x\ .\ \overline{\dot x^2+\dot y^2}^{\frac{3}{2}}}{\dot x^3\ddot x-\dot x^2\ddot x-\dot y^2\ddot x}=\frac{-\dot y\dot x}{\ddot x}\times\frac{\overline{\dot x^2+\dot y^2)}^{\frac{3}{2}}}{\dot x^3-\dot x^2-\dot y^2}$.

It is to be remarked, that all those expressions for the radius of curvature are strictly true and general; yet being in terms of quantities whose values would be extremely difficult to find, are not so applicable to practice as that containing only the fluxions of the absciss and ordinate. The entry of second fluxions into the definitive expression, does

Radius of Curvature



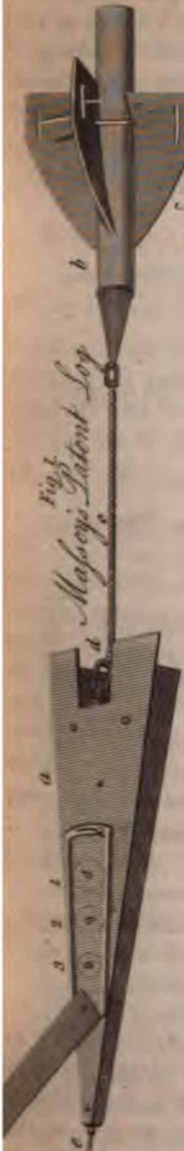


Fig. 1. Massey's Patent Log

Massey's Sounding Machine

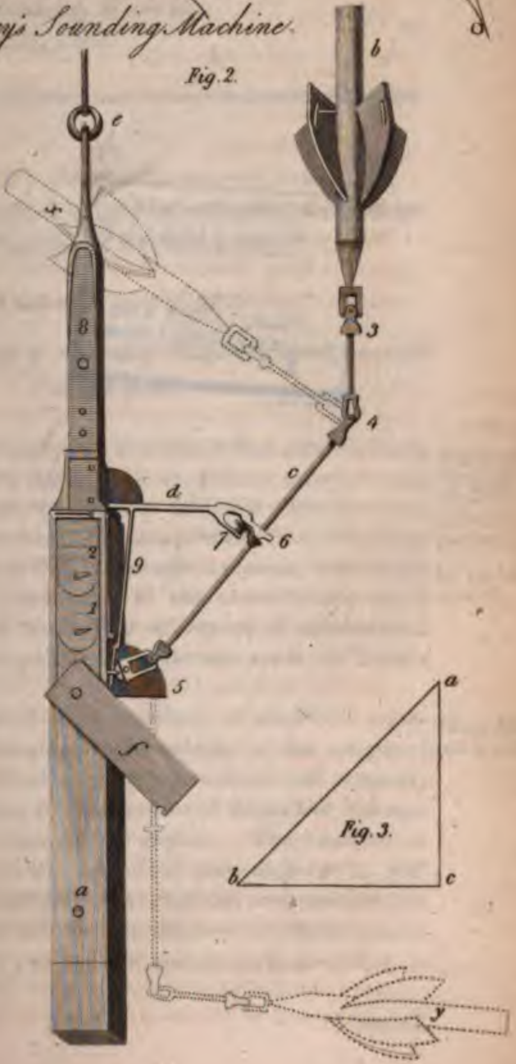


Fig. 2.

Fig. 3.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

does not imply, that the *nature* of the problem necessarily requires it: it arises from the particular artifice which is employed in finding the value of v, the ordinate of the circle; and is a matter of mere commodiousness, suggesting no other reason for their appearance, than that of a necessary consequence of such a particular step.

Royal Military Academy, Woolwich,
Oct. 13*th*, 1808.

IV.

Essay on the Composition of Alcohol and of Sulphuric Ether.

By THEODORE DE SAUSSURE.

(Continued from p. 231.

SECT. III. *Analysis of alcohol by detonating its vapour with oxigen gas.*

The whole of the alcohol not burned in the preceding experiments.

Its vapour detonated.

IN the preceding analysis I remarked, that alcohol, burning in a lamp under a closed receiver, diffuses a vapour, that has an alcoholic smell; it is very probable therefore, that the whole of the combustible disappearing from the lamp does not burn. Accordingly I sought a process, that should effect a complete combustion of the alcohol; and this I found in detonating a mixture of vapour of alcohol and oxigen gas over mercury, by the electric spark, in Volta's eudiometer.

This a difficult process.

This process applied to the analysis of alcohol is somewhat complex. It requires a knowledge of the weight of the vapour of alcohol at a given temperature and pressure, and the determination of the increase of volume of the oxigen gas by the presence of the vapour. The experiment must be conducted at a temperature exceeding 15° R. [66° F.] to obtain sufficiently decisive results; and neither the thermometer nor the barometer must vary during the course of it, which requires practice and quickness in several of its manipulations.

Method of computing the dilatation of vapour of alcohol.

I washed the inside of a large bladder with alcohol several times, letting the alcohol stand in it a long time, to take up every thing soluble in it, that this might not affect the expansibility. When this came out perfectly pure, the bladder was three parts filled with atmospheric air, two ounces of alcohol were poured in, and it was stopped with a cock. The air contained in it was dilated by the formation of alcoholic vapour. At the expiration of eighteen hours I fitted to the cock an empty receiver intended to weigh the air. The cock being turned, the dilated air passed alone, without any liquid alcohol, into the receiver, which was weighed before and after, the thermometer being at 17° [68° F.], and the barometer at 26 inches 9 lines during these operations and those that followed.

Twice repeated.

By this experiment, repeated twice under these circumstances, I found, that 1000 cubic inches of atmospheric air dilated by alcoholic vapour weighed 433·78 grains; and 1000 inches of the air employed in the experiment weighed before the introduction of the alcohol 424·5 grains.

Dilatation of the gas calculated by Mr. Dalton's formula.

To measure the dilatation the air had undergone by the alcoholic vapour, I employed the formula of Mr. Dalton, and passed into a barometer a drop of alcohol, which sunk the barometer 20·5 lines, expressing the elastic force of the vapour in vacuo. Applying this result to the formula $\frac{p}{p-f}$, where $p = 26$ inches 9 lines, and $f = 20{\cdot}5$ lines, we find, that, the volume of undilated air being equal to 1, it becomes 1·0682 by the conversion of alcohol into vapour; and as 1·0682 : 1 : : 1000 : 936·14, it may be inferred, that 1000 cubic inches of atmospheric air alcoholized contain 936·14 of atmospheric air. These weigh 397·4 grains; and as the alcoholic vapour occupies the same space as the air dilated by it, it follows, that 1000 cubic inches of pure vapour of alcohol weigh 433·78 — 397·4 = 36·38 grs.

1000 cub. inches of alcoholic vapour weigh 36·38 grs.

Vapours diffuse themselves in equal quantity whatever the gas.

I need not remind the reader, that, according to Dalton's experiments, vapours diffuse themselves in the same quantity through every gas, that has no chemical action on them*. I chose atmospheric air for finding the weight of the

---

Atmospheric air very slowly altered by alcohol.

* I kept atmospheric air in contact with alcohol a long time in a jar over mercury. In five months the air had undergone no sensible change, but in twelve it had lost ·01 of oxigen gas.

the vapour, because I could not employ pure oxigen gas in large quantity otherwise than at the point of extreme moisture; and if it had got dry, or if the external air had any way penetrated into the bladder, there would have been some errour in the calculation of the weight. I repeated the experiment however with oxigen gas, and found only a trifling difference, which I ascribe to the causes just mentioned.

Alcoholic vapour mixed with oxigen gas

To effect the combustion of alcoholic vapour, I prepared alcoholized oxigen gas, by passing some drops of alcohol into a jar filled with oxigen gas over mercury. I afterward withdrew the superfluous alcohol, that could not rise in vapour, by introducing dry unsized paper, and taking it out through the mercury, repeating this operation till the paper came out perfectly dry, and then emptying the dilated gas into a fresh jar. I had previously satisfied myself, that unsized paper would not condense the vapour of alcohol.

would not detonate by the electric spark

without a mixture of hidrogen gas, or a little liquid alcohol.

This alcoholized oxigen gas was introduced into a Volta's eudiometer filled with mercury, but I could not set it on fire by the electric spark. I was equally unsuccessful on adding pure oxigen gas in various proportions. The alcoholic vapour was too much rarefied in the oxigen gas to take fire. When I added a very small portion of hidrogen gas to the alcoholized oxigen gas, the electric spark produced complete combustion of the alcoholic vapour. The same effect took place, when I substituted an inappreciable quantity of liquid alcohol instead of hidrogen gas. The vesicular vapours, produced no doubt by this alcohol, answered the purpose of hidrogen gas: but in an accurate experiment this addition of liquid alcohol was inadmissible, as it was impossible to ascertain its quantity.

The experiment described.

Accordingly to 500 parts by measure of alcoholized oxigen gas I added 99·2, or near a fifth, of hidrogen gas, and detonated the mixture. The combustion, taking a mean of three experiments, gave a residuum, which, being analysed by lime water and by Volta's eudiometer, contained 342·59 parts of oxigen gas, and 46·69 of carbonic acid gas. I omit the nitrogen, which was found mixed in a small quantity with the oxigen gas both before and after the combustion, and acts no part that can be estimated. I must observe, that, when I opened the eudiometer immediately after the detonation,

tion, and while it was full of fumes, these were perfectly void of smell.

Calculation of the products.

The 500 parts of alcoholized oxigen gas contained before the combustion, according to the expansion of alcoholic vapour, only 468·07 parts of oxigen. The alcoholic vapour therefore and hidrogen gas added occasioned the disappearance of 468·07 — 342·59 = 125·48 parts of oxigen gas. The hidrogen gas added condensed half its bulk, or 49·6 parts. The 500 parts of alcoholic vapour therefore employed in their combustion 125·48 — 49·6 = 75·88 parts, forming 46·69 parts of carbonic acid gas, and a certain quantity of water.

If we consider the parts above mentioned as cubic inches, and to 500 of these substitute their equivalent weight of alcohol, we find, that 18·19 grs, of alcohol consume in their combustion 75·88 cubic inches of oxigen gas, forming some water, and 46·69 cubic inches of carbonic acid gas. These results by a similar calculation to that made for the slow combustion of alcohol, sect. II, show that 100 parts of this liquor contain

Elements of alcohol.

Carbon	48·82
Hidrogen	15·82
Oxigen	41·36
	100.

These elements may be deduced from the following expression. Ten grains of alcohol consume for their combustion 38·54 cubic inches of oxigen gas, the thermometer being at 28 inches and the thermometer at 10° R. [54·5° F.], forming water, and 23·67 cubic inches of carbonic acid gas.

This analysis more accurate than the former.

This analysis, in which the whole of the alcohol was consumed, must be more accurate than that made by slow combustion, sect. II. I shall presently show, that a small quantity of nitrogen is to be included in both.

SECT. IV. *Examination of the water produced by the combustion of alcohol.*

Alcohol in burning forms water.

Boerhaave and Geoffroy observed, that the vapour formed by the combustion of alcohol was water. Lavoisier found by means of an apparatus invented by Meusnier*, that the weight of this water exceeded that of the alcohol consumed. In this process all that is formed is not collected, because this process is conducted in open vessels, in which the air is continually renewed by a rapid current, that carries out of the apparatus a considerable portion of the vapour before it has time to condense. In burning 100 parts of spirit of wine Lavoisier collected about 116 parts of water†. My analysis, sect. III, shows, that this aqueous product should amount to 132 parts for 100 of perfect alcohol: but Lavoisier did not employ this, which would have afforded a result nearer to mine. As it is impossible to make this comparison with accuracy, I contented myself with examining whether the water produced by this process were pure.

100 p. alcohol produce 132 water.

The water examined.

The water obtained from alcohol by the apparatus of Meusnier, or more simply by burning it in the open air under the mouth of a large glass receiver, which condenses the aqueous vapours on its sides, so that they drop from its mouth, has not the alcoholic smell observed in the product of combustion under a close receiver, sect. II; because in the latter the alcoholic vapour is retained, while in the open air it is dissipated, leaving as a residuum only the less evaporable fluid with which it was mingled.

Its properties.

This liquor is insipid: it has the same specific gravity as distilled water: it does not change the colour of sirup of violets or of infusion of litmus: it is not precipitated by acetate of barites, nitrate of silver, or limewater.

Residuum left by it.

Two ounces of water obtained from the combustion of alcohol in the open air under the mouth of a glass receiver were evaporated to dryness, and left as a residuum a thin transparent varnish, that weighed $\frac{1}{5}$ of a grain, and attracted moisture from the air. The solution of this varnish in a small quantity of water was rendered slightly turbid by oxalate of potash. The combustion of spirit of wine rectified without addition afforded the same result. This residuum appeared

The same by alcohol rectifed without addition.

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* For a description of this apparatus see Lavoisier's Elémens de Chimie, vol. II, p. 189, 1st edit.

† Lavoisier's (posthumous) Mémoires de Chimie, vol. II, p. 281.

appeared to me owing in part to the lime and potash, which I have found in the ashes of alcohol by other experiments. They are held in solution by acetic acid formed by the combustion. This water, kept in a phial half filled with it, after some months deposited a slight mouldiness.

The water grew mouldy.

Muriatic acid elicits ammoniacal vapours from it.

At the approach of muriatic acid this fluid diffuses copious ammoniacal vapours. This effect is more striking, when the water has been collected by Meusnier's apparatus, because in this process the ammonia, or rather the acetate of ammonia, has less time to evaporate. That I might not be mistaken with respect to the nature of these vapours; and to collect a part of the ammonia, which is volatilized and lost in the atmosphere in proportion as the water is produced; I poured a few drops of muriatic acid into the phial, which in Meusnier's apparatus is employed to receive the liquid formed by the combustion. After having obtained 4½ oz. of this water, which was thus mixed with muriatic acid, I subjected it to spontaneous evaporation in a place where I could not suspect the presence of any ammoniacal vapours, and obtained a residuum containing 3¾ grs. of muriate of ammonia, perfectly characterized by its crystallization and other properties. It was at first mixed with a small quantity of muriate of lime and muriate of lead*: the deliquescent salt was separated by elutriation; and the insoluble metallic salt by dissolving the residuum in distilled water.

With this acid it forms muriate of ammonia.

Greater part of the ammoniacal gas lost.

I could not judge by this result of the quantity of nitrogen contained in alcohol, because the vapour of muriatic acid formed a smoke of muriate of ammonia, the greater part of which escaped out of the vessel employed to receive it.

The ammonia not produced by the azote in the air.

It is not probable, that this ammonia was owing to the combination of the hidrogen of the alcohol with the nitrogen of the atmospheric air, for it has been seen, sect. II, that the latter was not condensed in the combustion of the alcoho

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Lead dissolved from the worm.

* The worm of my apparatus is of lead. In this case the water produced by the combustion of the alcohol held the metal in solution probably by means of acetic acid. The water thus obtained gave a black precipitate with hidrosulphuret of potash, even when there was no muriatic acid in the receiver; but it did not produce this effect, when it was collected from alcohol burned under a glass jar.

alcohol. Besides it will be shown by more direct observations, perfectly free from objection, that alcohol contains this element.

This result is of importance to the theory of fermentation.

Azote an essential part of yeast.

Mr. Thenard had remarked*, that the nitrogen, which is an essential part of yeast, disappears in the vinous fermentation. This element was not then found among the products of this process, but we shall see, that it enters into the alcohol.

The ammonia neutralized by acetic acid.

The ammonia contained in the liquid formed by the combustion of alcohol appears to me neutralized by acetic acid. I have poured a few drops of potash into two ounces of this water. The alkali, which was in excess, was saturated by carbonic acid, and slightly dried in the open air. I washed the whole with alcohol, and the decanted liquor afforded by evaporation a very deliquescent salt, which had all the other properties of acetate of potash, and weighed 1¼ grain.

The water from both kinds of alcohol contained ammonia, acetic acid, lime, and probably potash, but in quantities of no importance.

All the trials I have just mentioned of the water obtained from perfect alcohol, repeated with water obtained from spirit of wine rectified without muriate of lime, gave the same results. They showed, that it contained ammonia, acetic acid, and lime, and probably a little potash: but all these substances were in such small quantity, that they could not have much influence on the proportions of oxigen, hidrogen, and carbon, assigned to alcohol by my last analysis, sect. III, where I considered the fluid formed by burning it as pure water.

SECT. V. *Analysis of alcohol by means of a redhot tube of porcelain.*

Analysis of alcohol by passing through a redhot porcelain tube.

Several chemists have noticed with more or less accuracy the nature of the principal products afforded by alcohol in passing through a tube of porcelain heated red hot. They have observed water, oxicarburetted hidrogen gas, and carbon; and lastly Mr. Vauquelin mentions a crystallized volatile oil†: but they have not obtained from these products a detera determination of the number and proportion of the elements of alcohol. I have attempted however, to attain a knowledge of these by the same process.

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* Essay on Vinous Fermentation by Thenard: Annales de Chimie vol. XLIX, p. 294: or our Journal, vol. VII, p. 33.

† Fourcroy's Chemistry, vol. VIII, p. 155; or English edition, p. 207.

Process described.

Through a red hot tube of porcelain, glazed internally, I distilled 2183 grs. of perfect alcohol. The products passed from this tube into a glass worm* surrounded with cold water, and thence into a small globular receiver, which retained the liquid products, and allowed the gasses to pass on to the pneumatic trough.

The retort, which introduced the alcoholic vapours into the porcelain tube, was kept at a temperature between 40° and 50° R. [122° and 144° F.]. The distillation continued twenty hours. I conducted it slowly, that scarcely any of the alcohol might escape decomposition in traversing eight inches of redhot tube. From this process I obtained,

Results. Charcoal,

1. In the porcelain tube $4\frac{1}{4}$ grs. of charcoal, which separated in the form of a thin film rolled up like a scroll, and several inches long. This charcoal, being incinerated in a platina crucible, left about a grain of ashes, in which I discovered, by lixiviation with water and solution in muriatic acid, the presence of potash, lime, and an insoluble residuum, which might be silex. Mr. Proust had already found silex and lime in alcohol.

with a little potash, lime, and perhaps silex.

Concrete essential oil,

2. The glass worm was lined with the crystallized essential oil discovered in this process by Mr. Vauquelin. These crystals presented themselves to the naked eye in the form of thin, transparent, white, and yellowish scales: but with the microscope some of them exhibited quadrilateral prisms with diedral summits. They are very soluble in alcohol; and the solution becomes milky on the addition of water, if the alcohol be not too abundant. These crystals, as well as a very thick brown oil with which they are mingled, and which is scarcely volatile at the common temperature, have a strong smell of benzoin. The weight of these two oils collected and added together, both in the worm and in the receiver, amounted to 4 grains. The receiver contained but half a grain.

and a thick brown oil, both smelling strong of benzoin.

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* When I used a leaden worm, the liquor passing through it held some lead in solution.

3. I

Water with a little alcohol,

3. I found in the receiver, beside this small quantity of oil, 196 grs of colourless water, the specific gravity of which was 0·998, indicating a mixture of 193½ grs of water, and 2½ grs of alcohol. These 2½ grs therefore are to be deducted from the 2183 subjected to analysis.

smelling of benzoin and vinegar, reddening blue tests, and emitting ammoniacal vapours.

The water I have just mentioned had a smell both of benzoin and of vinegar: it reddened sirup of violets, and infusion of litmus: it diffused ammoniacal vapours at the approach of muriatic acid: it was not precipitated by lime water, or by nitrate of mercury, but was rendered slightly turbid by nitrate of silver. This circumstance, added to the smell of benzoin, led me to suspect the presence of benzoic acid.

Analysed,

To find the quantities of the foreign principles contained in this water, I added it to a similar liquid obtained by the same process in another trial, and divided the mixture into three parts of 100 grs each.

it left on evaporation a slight residuum,

The first, evaporated to dryness in the temperature of the atmosphere, left at the bottom of the vessel a transparent varnish incapable of being weighed.

with carbonate of potash effervesced & formed acetate,

The second portion was mixed with crystallized carbonate of potash, which dissolved in it with effervescence. The solution, evaporated to dryness, left a residuum, on which I poured alcohol. The decanted liquor left by evaporation a white salt, which on exposure to the air speedily resolved itself into a fluid, except an infinitely small quantity of a salt in stellar crystals, resulting probably from a union of the potash with the acid that precipitated the nitrate of silver. The saline substance that deliquesced was acetate of potash. Its quantity in the dry state would have been for the 196 grs of liquid I examined 0·9 of a grain, which indicates 0·55 of a grain of glacial acetic acid in the whole aqueous product of this analysis.

and with muriatic acid gave acetate of ammonia.

Lastly, the third portion was mixed with muriatic acid, to saturate the ammonia. This mixture furnished by evaporation crystals of muriate of ammonia, but the quantity was too small to be weighed.

Its contents.

From this examination the 193½ grs of water obtained from the decomposition of the alcohol by a red hot tube contained acetic acid in excess, ammonia, and probably benzoic acid: but as the weight of all three together amounted to about $\frac{1}{400}$ only of the fluid that held them in solution, it may be considered as pure water, without any risk of errour, in an analysis like the present.

Oxicarburetted hidrogen gas.

4. The oxicarburetted hidrogen gas, the barometer being at 27 inches, and the thermometer at 17° R. [70¼° F.], occupied the space of 7199 cubic inches; and weighed, the day after it was collected, taking a mean between the weight of the gas that came over at the beginning, middle, and end of the process, 1786·61 grs*. Though the heat of the tube did not perceptibly vary, the gas obtained at the beginning of the experiment was lighter, and contained less carbon, than at the end. This was owing to the charcoal deposited by the alcohol accumulating gradually in the tube, and reacting on the fluid that was decomposed in proportion to this accumulation. However slowly I conducted the distillation, I could not prevent the gas from carrying over with it pretty copious white fumes, the weight of which I could not directly calculate, and the loss of which occasioned a deficiency in the results of the analysis. These fumes smelled of benzoin; and appeared to me to afford on condensation similar products with those collected in the receiver, namely, a great deal of water, and a very small quantity of oil. The latter could only be in very small proportion; for, on detonating the gas immediately after its developement, and while these fumes were suspended in it, I did not obtain more carbonic acid gas from the combustion, than when it was detonated after the fumes had been condensed in the water under the jars. The un-

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Composition of the gas affected by the manner of conducting the process.

[*] At 28 inches of the barometor therefore, and 10° [54½°] of the thermometer, 1000 cubic inches of this gas weigh 266 grs. This result differs a little from that of Mr. Cruikshank, who makes it 237 in the same circumstances. I have performed this experiment three times, changing the diameter of the tube a little, and likewise its inclination in the furnace, and each time I found a perceptible difference in the weight of the gas and its composition. But the sum of all the products, in each of the experiments, afforded similar results for the composition of alcohol. Thus it appears, that we should be liable to considerable errour, if we did not compare together all the products of each experiment.

certainty left by the composition of this vapour however can affect only an 11th part of the alcohol subjected to analysis.

Immediate products of the decomposition of the alcohol.

On adding together the weights of the immediate products of the whole process, we find, that 2180·5 grs of alcohol afforded

Gas	1786·61 grs
Water	193·50
Oil	4
Charcoal	3·25
Ashes	1
	1988·36
Deficiency from fumes, chiefly aqueous	192·14
	2180·5

Analysis of the oxicarburetted hidrogen gas.

Analysis of the oxicarburetted hidrogen gas.

The 7199 cubic inches of this gas contained no carbonic acid gas. They were collected in eighteen jars, all of which were examined eudiometrically. I shall give here the mean of these eighteen analyses, deducting the atmospheric air contained in the vessels previous to the distillation. With 100 parts of the oxicarburretted hidrogen gas were mixed 200 of impure oxigen gas, consisting of 190 oxigen and 10 nitrogen. The mixture inflamed by the electric spark left for a residuum some water, and a mixture of carbonic acid gas, oxigen gas, and nitrogen gas, occupying together the space of 156·5 parts. These were washed with lime water, and analysed afresh by Volta's eudiometer, adding to them hidrogen gas. I thus found, that they contained

Carbonic acid gas	78
Oxigen gas	65·93
Nitrogen gas	12·57
	156·5.

These results show, that the 124·07 parts of oxigen gas, which disappeared to effect the combustion of 100 parts of oxicarburetted hidrogen were employed to form 78 parts of carbonic acid gas, and to burn (124·07 — 78) × 2 = 92·14 parts of hidrogen gas belonging to the oxicarburetted hidrogen gas. Thus we find, that 100 parts of the latter contain 2·57 parts of nitrogen gas. If by the rule of proportion we estimate from this the results of 7199 cubic inches of oxicarburetted hidrogen gas, weighing 1786·61 grs, we shall find, that they would have produced by their combustion 5615·2 cubic inches of carbonic acid gas, containing 945·59 grs of carbon; that the oxigen gas would have burned 6633·2 cubic inches of hidrogen gas, weighing 212·44 grs; and lastly, that the whole of the oxicarburetted hidrogen gas contains 185 cubic inches of nitrogen gas, weighing 76·77 grs.

Analysis of the oxicarburetted hidrogen gas.

If we add together the weight of the elements just calculated, we shall have, in 1786·61 grs of oxicarburetted hidrogen gas,

Carbon	945·59
Hidrogen	212·44
Nitrogen	76·77
	1234·80
Deficiency	551·81
	1786·61.

The residuum of the combustion of the oxicarburetted hidrogen gas appeared to me to be nothing but water, excepting the carbonic acid gas and nitrogen, that have been mentioned. Thus the deficiency we find on adding together the elements of this analysis must be ascribed to the elements of water, which existed in the oxicarburetted hidrogen gas not in the state of water or aqueous vapour, but in a state in which they were united and as it were confounded with the other principles of this gas. If we substitute for this deficiency therefore the elements of 551·81 grs of water, we shall find, that the 1786·61 grs of oxicarburetted hidrogen gas are composed of

Carbon

Its constituent principles.

Carbon	945·59
Oxigen	485·59
Hidrogen	278·66
Nitrogen	76·77
	1786·61*

Carbon in the alcohol.

To come at the whole of the carbon contained in the 2180·5 grs. of alcohol I decomposed, we must add to the 945·59 grs. of carbon in the inflammable gas the 3¼ grs. from the charcoal found in the porcelain tube, and that of 4 grs. of oil which might amount to about 3 grs. These added together make 951·84; and thus 100 parts of alcohol contain 43·65 of carbon.

Oxigen in the alcohol.

To find all the oxigen of the alcohol, we must add to the 285·59 grs. of oxigen belonging to the inflammable gas the oxigen of 193·5 grs. of water in the receiver adapted to the worm. Thus the sum of oxigen was equal to 485·59+170·28 =655·87 grs. From 100 parts of alcohol therefore we should have 30·12 of oxigen.

Hidrogen in the alcohol.

To obtain the whole of the hidrogen of 2180·5 grs. of alcohol, we must add to the 278·66 of hidrogen found in the oxicarburetted hidrogen gas the hidrogen of the 193·5 grs. of water collected in the receiver, and the hidrogen of 4 grs. of oil, which might be about 1 grain†. The sum of these is 302·88 grs.; so that 100 parts of alcohol would have furnished 13·89 grs. of hidrogen.

Nitrogen and ashes.

Adding to these elements the quantity of nitrogen I found in the inflammable gas, and lastly that of the ashes obtained

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* This gas therefore contains in 100 parts by weight,

Carbon	52·9
Oxigen	27·2
Hidrogen	15·6
Nitrogen	4 3
	100.

† This oil does not make the five hundredth part of the weight of the alcohol I decomposed: so that in the present analysis, which is merely an approximation, I might have neglected this product; and therefore it is of little consequence, whether the composition I ascribe to it be just.

by the incineration of the charcoal, we find, that 100 parts of alcohol produced.

Sum of these.

Carbon	43 65
Oxigen	30·12
Hidrogen	13·89
Nitrogen	3·52
Ashes	0·04
	91 22
Loss	8·78
	100.

Deficiency to be supplied.

I noticed at the commencement of this analysis, that this loss was owing to fumes that contained a great deal of water, and an infinitely small quantity of oil, carried into the pneumatic trough by the oxicarburetted hidrogen gas. If for this loss we substitute 8·78 parts of water, we shall find, that 100 parts of alcohol contain.

Real proportions of the elements.

Carbon	43·65
Oxigen	37·85
Hidrogen	14·94
Nitrogen	3·52
Ashes	0·04
	100·

Results agree with those in Sect 2.

The results of this analysis are nearly similar to those I obtained by the detonation of the elastic vapour of alcohol in a Volta's eudiometer, section III, setting aside the nitrogen, which I could not calculate in that process, and which remained confounded with the water in the state of ammonia, if not almost wholly with the 41·36 parts of oxigen, which that analysis ascribed to the alcohol. If from these 41·36 parts of oxigen we substract the 3·52 of nitrogen we have just found, the two analyses will agree better than could have been expected from such a complex process.

Alcohol rectified alone gave similar results.

I analysed by means of a red hot tube spirit of wine rectified by simple distillation, and found no difference of importance between the two results, when I deducted the quantity of water in this spirit calculated from its specific gravity.

(To be concluded in our next.)

V.

Description of an improved Mode of constructing Muffles for Chemical Purposes, by Mr. Edmund Turrell, *No.* 40, *Rawstorne-Street, Goswell-Street Road*[*].

Common mode of moulding muffles inconvenient.

HAVING experienced much inconvenience in the common mode of moulding muffles on wooden blocks, for the use of chemists, enamellers, &c., I beg leave to lay before your praise-worthy Society, an improved method, possessing the following advantages: namely,

Advantages of the new mode.

First, By this new method of moulding muffles, coarser and cheaper materials may be used than can be employed in the common mode; and which also gives them the valuable property of resisting a greater degree of heat.

Secondly, That much time will be saved by this improved method of manufacturing them, must be allowed, when the two modes are compared.

Thirdly, The certainty of making them without cracks or flaws, and with coarser materials, will appear obvious, when it is considered, that by this improved method, they are *internally* moulded instead of *externally;* by which means the strength of the operator may have its full effect, in firmly compressing the composition into the mould.

Disadvantages of the old mode.

In the old mode, the workman, after having spread the composition upon a cloth, guessing at its thickness, bends it over the block in the best way he can; and by thus disturbing the composition, he must needs make many cracks and flaws, which can be but imperfectly closed in smoothing the surface of the muffle while upon the block; the evil consequence attending which is, its being subject to fly or crack when exposed to a great heat; and it will also be plainly seen, that, in the old mode, a great disadvantage is felt by the sides of the muffle, while in its wet state, hanging from its centre, which also tends to crack it, as there can be nothing applied to assist it in this case, but by employing a greater proportion of cohesive clay in the composition, which, however, produces little if any advantage; whereas in the mode which I have invented, this fault is entirely obviated, and the composition, by its contraction in drying, assists the extrication of the muffle from the mould.

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[*] Transactions of the Society of Arts, for 1807, p. 38. Ten guineas were voted to Mr. Turrell for this invention.

Farther advantages of the new method.

Fourthly, With respect to simplicity, this new mode will be found to possess a very great advantage, for a boy of twelve years of age may be taught to make them in a very short time.

The fifth advantage of this improvement, and of equal consideration, is the cheapness of the article; the price of which has been reduced nearly one third to the consumer; and when the superior quality of them is taken into consideration, it may fairly be said to be one half. I mean, when regard is had to their superior quality; and that the muffles may be used over again when broken and ground, with a much less proportion of cohesive clay than in the old mode; and this I conceive to be no inconsiderable advantage; for it is well known, that when the old muffles or broken crucibles can be used without much fresh clay, they are far superior to new materials.

Sixthly, The muffles made in the old way are seldom of equal thickness; whereas those made according to the method which I have the honour to present before the Society, will be found to possess that necessary quality in perfection; for, if a hundred are made from the same mould, they will be all of the same thickness.

Description of the Moulds and Implements.

Description of the method of making the muffles.

The first mould for this purpose is a tin one, Plate VIII, fig. 1, which may be made from a piece of tin the size of the arch, being bent so as to form such a concavity as may best suit the purpose to which it is to be applied. This being done two square pieces of tin, *a a*, must have an arch cut out of them, of such a size that the diameter thereof may be about three fourths of an inch less than the diameter of the concave piece before stated; these, being soldered to each end of the first-mentioned piece, will form a stand for the hollow part of the mould, and the thickness of the muffle moulded in this will be exactly determined by the edge at each end. A piece of hollow tin, *b b*, may be soldered along the top edge of the mould, to form a better resistance to the great pressure within. The next part of this mould is a flat piece of tin, fig. 2, cut exactly to fit the inside of the mould, the use of which is, to form a solid back to the muffles used for chemical purposes.

Description of the method of making the muffles.

The second tool for this purpose is a piece of sheet brass, fig. 3, about six inches long and one broad, which, being bent in a semicircular form, and screwed to a piece of wood extending beyond its breadth about an inch, is used for cutting the small air holes *c* (fig. 11), in the aforesaid muffles.

The third is the tool or frame, fig. 4, for preventing the contraction of the muffles in drying, which is made of four pieces of beech, about three quarters of an inch broad, and half an inch thick; the length must be adjusted to the mould of the muffle; two of these being laid parallel within the inside of the mould, and being joined across by the other two, the ends of which should extend so far beyond the outer edges of the other two, that they may rest upon the edges of the muffle mould, and thereby prevent its falling into the mould.

The fourth is the tool for spreading the composition into the moulds, which is formed of iron or steel, (fig. 5), about thirteen inches in length, one inch and a half broad, and about one eighth of an inch thick; its face under *h* being rounded in such a manner, that its curve may exactly fit the inner curve of the muffle mould, (fig. 6, is a section of it). This should likewise have a point or tongue, extending from each end, long enough to be bent in the form of a bricklayer's trowel, and by the wooden handles which must be put on, hanging down, it will be found, that, as it is moved either backwards or forwards, it will always present an edge to smooth the composition, and condense it in the mould.

The fifth is a frame (*d d*), fig. 15, of which the bottom and

T 2 farthest

Description of the method of making the muffles.

farthest side are only shown, and in which frame the tin mould, fig. 1, is placed, simply constructed by joining two pieces of wood, the one as broad as the bottom of the muffle mould, and having two narrow grooves (*e e*), cut in it, so that the edges of the tin mould may be confined therein. The other board, being joined to this at its edge, should come up so high as just to be under the edge of the mould.

The sixth is the tool for cutting the muffles of different lengths (fig. 7), and is made of a piece of wood, to the end of which is fixed a thin piece of brass (*f*), which, extending about one inch and a quarter beyond the top of the wood, is bent at right angles, and made thinner at the end, that it may the more conveniently cut the muffle. Under this piece of wood is used another straight piece (*g*), with two steady pins, which, being shifted at the will of the workman, will cut them of any length.

The seventh is the mould for forming the bottom of the close muffle (fig. 8), which is made of a mahogany or oak plank, about sixteen inches long, ten wide, and about three eighths of an inch thick; upon this is fixed a ledge on each side, one inch broad, and nearly half an inch thick, and at each end a ledge of the same kind is placed, at such a distance as is best suited to the length of the bottom required. Fig. 9 and 10, are circular moulds for muffle bottoms of dial plates. Fig. 11, a complete muffle standing on its bottom. Fig. 12, a roller for rolling the composition in the first mould. Fig. 13, a tool for making small holes in the muffle.

The usual composition for making muffles is as follows: viz. two parts pipe clay and one part sand, such as is used by the bricklayers, sifted, and mixed together to a proper consistence; this is very expensive, on account of the high price of pipe clay, which is about ten shillings the hundred weight, whereas I employ in my improved mode of making them the coarser kind of Stourbridge clay, which can be had at the glass-houses, in the ground state, for six shillings the hundred weight; and this I sift also, to separate the finer part, which I employ for making other smaller articles necessary in my business; using only the grosser or coarser part for muffles, to which I add one eighth part only of pipe clay, mixing them

wel

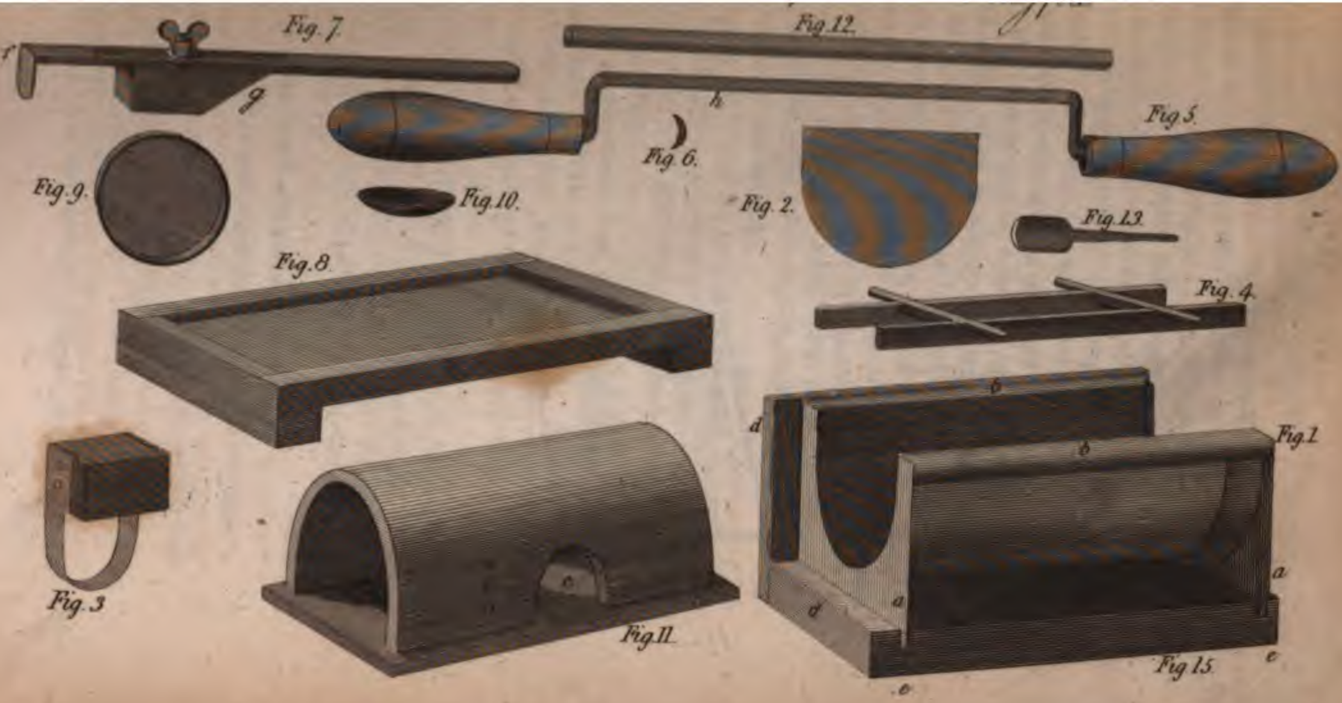

Fig. 7.

Fig. 12.

Fig. 5.

Fig. 6.

Fig. 2.

Fig. 13.

Fig. 9.

Fig. 10.

Fig. 8.

Fig. 4.

Fig. 1.

Fig. 3

Fig. 11.

Fig. 15.

Certificates from Messrs. J. Haynes and Son, Westmoreland Buildings; John Kelly, Hooper-Street, Clerkenwell; John Foster, Author Street, St. Luke's; and William Foster, Author Street, St. Luke's, states, that they have been in the habit of using Mr. Turrell's muffles for upwards of twelve months, that they are greatly superior to any they have hitherto been able to procure; and that it is their opinion their durability may be completely attributed to his improved method of moulding them.

VI.

Considerations on the State in which a Stratum of nonconducting Matter must be, when interposed between Two Surfaces endued with opposite Electricities: by A. AVOGADRO, *Corresponding Member of the Academy of Sciences at Turin*[*].

SECTION I.

State of a nonconductor between two conductors not sufficiently examined.

THOSE learned natural philosophers, who have lately studied with so much success the mechanism of the forces, that electricities of the same or opposite kinds exert on each other, either in conducting substances, or through nonconductors, have not paid equal attention to the facts, that may lead us to some knowledge of the state of the insulating substance[†] through which these forces act; particularly when it is interposed between two electricities of opposite kinds, that mutually support each other by their attraction. Yet if such facts exist, they might lead us to consequences of great importance to the theory of electricity, In reality the circumstances I have just mentioned, the interposition of an insulating stratum between two bodies endued with opposite electricities, is of great extent in electrical phenomena: it not only takes place with respect to substances made to approach each other expressly for this purpose, and in charging the Leyden phial, or a plate of a glass, which in fact is nothing more than a method of bringing the two coatings nearer together than could be done in the air: but it introduces itself generally into all the electricity of conducting substances, as is easy to be observed; for every electrified body is surrounded with other bodies more or less distant, on the surface of which, according to the established principles, the electricity of the former body can only occasion an opposite electricity by acting through the intervening stratum of air. We may truly say, therefore, that there is no electricity but has opposite to it the contrary electricity, with an intermediate insulating stratum.

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[*] Journal de Phisique, vol. LXIII, p. 450.

[†] In this paper I employ the term *insulating* as synonimous with what is commonly called *nonconducting*, or *electric per se.*

Leyden phial.

A general case.

On the other hand, were we once convinced of the insulating stratum in these circumstances being in a peculiar state, distinguishing it from a simple medium through which the electric forces exert themselves, it is natural to suppose, that a more attentive examination of this state would give us some idea of the manner in which those forces exert themselves, which the labours of philosophers have hitherto only confirmed; but which, according to all appearance, are not to be ascribed to an original property inherent in the substances that exert them, as has been asserted with great probability in respect to the Newtonian attraction of matter in general.

Electricity not an inherent quality like gravitation.

Now reflecting on certain facts, that Symmer, Cigna, Beccaria, Volta, and others have established by their experiments, it has seemed to me, that we might deduce from them some inferences relative to the state of the insulating stratum in question. The object of the present paper is to detail the ideas, which these facts have suggested to me.

Facts leading to a knowledge of the state of the nonconductor.

SECT. II. The fundamental experiment*, which first presents

Fundamental

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* The facts adduced in this and the following section are not new; neither indeed are the reflections accompanying them wholly my own. Æpinus, Haüy, Volta, and others, have already given them at least in part, and in a form more or less resembling that in which I exhibit them; but perhaps they have not paid them sufficient attention in general. I have thought it necessary, to resume this subject in a somewhat more extended way, as an introduction to the ideas that constitute the principal object of this memoir, and which I begin to lay before the reader in section IV.

experiment when two non-conductors charged with opposite electricities come together, the electricities disappear.

presents itself in this branch of electrical science, is the following. When two insulating bodies, or one insulating and one conducting body, the surfaces of which are endued with opposite electricities, come to be applied to each other by their surfaces, the electricity of both seems to disappear. We no longer find any signs of it, or at least if any sign of either kind of electricity remain, they may easily be deprived of this surplus: but then if we endeavour to separate these two bodies, we find, that they adhere together, which proves, that all their electricity was not really destroyed; and if we overcome this resistance, and actually separate them, we shall find, that each of thess bodies again exhibits signs of that kind of electricity, which it possessed before they were brought together. These phenomena however are not to be observed in all their simplicity, except with insulating bodies of a texture sufficiently thin to be incapable of an electric charge; and such as have a kind of communication between their surfaces, as with two ribands for instance, or two silk stockings, or one of these and a piece of insulated tin foil. I shall not here enter into the particulars of these experiments, which may be seen in Priestley's History of Electricity, Mr. Symmer's communications to the Royal Society, Mr. Cigna's paper in the 3d vol. of Miscellanies of the Royal Society of Turin, &c.

But it is not destroyed.

Only a particular case of an acknowledged general law.

Before I introduce this fact into the examination, that constitutes the principal object of this paper, it may not be amiss to show, that it is merely a consequence of the known principles of electricity, a particular case of a law, the generality of which is at present acknowledged.

The thinner the interposed substance, the stronger the opposite electricities.

It is known, that one kind of electricity is capable of being so much the more condensed on a surface in proportion to the proximity of another surface endued with the contrary electricity, the attraction between the two kinds of electricity in this case surmounting with more advantage the repulsive power, which opposes this condensation in each kind of electricity. It is known likewise, that for this reason the coatings of a Leyden phial can acquire much greater quantities of electricity than bodies of equal surfaces electrified in the air; and it is a practical truth long known, and depending on the same principle, that the

Leyden phial.

phials, panes of glass, &c., have so much the greater capacity for a charge, other circumstances being equal, in proportion as their thickness is less.

Case of two electrified ribands.

This admitted, let us suppose, that the surfaces of our two ribands, endued with opposite electricities, are gradually brought nearer to each other, keeping them parallel, The repulsive force of each of these two electricities will render itself so much the more perceptible, and they will have so much the less tendency to be conveyed away by the surrounding bodies, as their distance is diminished; because the attraction between the two electricities will become so much the greater: and when at length the surfaces are brought into contact, the attraction having become as it were infinite, these electricities will no longer tend to fly off, but will remain as if they did not exist with respect to other bodies, whatever intensity they had before, since this intensity was limited, and the repulsion arising from it was also limited.

Charged plate of glass.

This may be exhibited in another point of view. When we charge a plate of glass, the electricity produced on the interior face of the coating opposite that which is electrified directly has no tendency to fly off, because it is perfectly retained by the attraction of the electricity of the latter coating; an electricity which, according to the principles of Coulomb and Haüy, to produce this effect must be conceived somewhat greater, than that which is produced on the opposite face of the plate. The electricity of the face electrified directly is on the other hand perfectly retained by that of the opposite face, with respect to the portion equal to it: and it is only its excess that has a tendency to be dissipated, and requires the resistance of the air to retain it. Now Haüy has already observed, that this excess, according to the theory, must be so much the less, in proportion to the thinness of the plate; and that it would be nothing, if the plate were infinitely thin, or in other words nought. Neither of the two electricities then, that compose the charge, would any longer tend to fly off; they would become insensible, This is precisely the case of the electricities of our two ribands, considered as coatings of the stratum of air at first interposed between them, and which becomes nought by their contact.

The two electricities not destroyed by the contact.

There is nothing here at which we need be surprised, except, that the two electricities are not destroyed by the contact, where nothing appears to prevent their mutual attraction from exerting itself. But we may suppose, that this attraction is sufficiently satisfied by the mere contact, that this contact serves instead of actual communication, and that it neutralizes the two electricities, as communication itself would do; for it is sufficiently proved, that this does not take place, and that the two electricities still belong to the two faces separately, since they immediately manifest themselves when the two surfaces are again separated so that a fresh stratum of air is introduced between them. The resistance we experience in this act of separation is likewise an effect of the two subsisting electricities, the mutual attraction of which necessarily opposes a separation of the surfaces, which carries with it that of the electricities.

Why the phenomena cannot take place between two conductors.

It is easy to perceive however why we cannot observe these phenomena between two conducting bodies; for as the contact of the two surfaces can never take place accurately and and instantaneously at every point, the first point of contact between two bodies of this kind is sufficient to destroy the whole electricity of the two surfaces, which still retains its intensity, and is not yet neutralized by contact. The same thing would take place on the separation of these bodies, even if we supposed them possessing this electricity, though imperceptible, in the state of contact. The retention and apparent reproduction of the electricities therefore cannot take place, unless one of the two bodies at least is an insulator.

As the electricity was supposed to be destroyed, nonconductors were supposed to reproduce it on separation.

Before I proceed farther I shall observe, that the total loss of intensity, which the electricities experience in the contact in question, has led some to imagine, that the electricities really destroyed each other by communication: particularly as they could not conceive what should prevent this communication from taking place: and in consequence they were obliged to suppose, that insulating bodies had the singular property of resuming on separation the electricity they had deposited on coming into contact; that they *reclaimed* it as it were: and hence the name of *claiming* electricity *(electricitas vindex)*, which Beccaria gave the electricity thus apparently reproduced. The adhesion of the two surfaces on their contact however had led others to presume, that the electricity was merely latent, and not annihilated.

The electricity is in fact only quiescent.

This question, which presented no clew to its elucidation, and which was reduced almost to a dispute about words, or to a different mode of viewing the same object, while the phenomenon in consideration was examined separately, solves itself now we perceive its connection with known principles: for it is demonstrated *a priori*, that the electricity in this case must lose its intensity, and become imperceptible, though it is not really destroyed: We may express this state by the term of *quiescent electricity*, or electricity at rest.

When the intervening non-conductor is thick the phenomena appear more complex.

SECT. III. Let us now pursue the inquiry. I have said, that it is only with insulating substances of a thin texture we can observe the phenomena of which I have spoken in all their simplicity. It is easy to conceive with respect to compact bodies capable of being carged with electricity, as for instance two plates of glass, that the dependance, which the electricity of one of the faces has or may have on the electricity of the face opposite to it, with which it forms or may form the electric charge of the plate, must necessarily render more complex the phenomena relative to the state of rest, and to the revivification of the electricity of the faces, that are brought into contact or separated. I shall not enter here into the details the subject would require. Though several natural philosophers have already engaged in researches of this kind, much remains to be done, to illustrate it completely*. Nothing more, is necessary for my purpose here,

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History of the experiments on this subject.

*The first experiments on quiescent electricity, and its revivification in compact bodies, were made by the Jesuits of Pekin, and communicated to the Academy of Petersburg in 1755. These gave occasion to a paper by Æpinus in the 7th vol. of the New Transactions of that Academy. Symmer treated the same subject in his fourth paper, read to the Royal Society in 1759. Lastly Beccaria entered into it very largely in his book entitled "Observationes atque Experimenta, quibus Electricitas vindex late constituitur, and explicatur. Turin, 1769. The reader may likewise see two plates of glass discharged as a single one.

here, than to relate the simplest case. Let us suppose, that, after having charged two plates of glass, those faces are brought into contact, which are charged with opposite electricities, being previously divested of their coating: and a communication between their exterior coatings is then established; in other words, that the two plates thus joined are discharged as if they were a single plate. The plates thus joined will no longer give any sign of electricity, and the two exterior electricities are destroyed by this communication, as if there were no others. As to the interior electricities, it seems at the first view, that they must have been annihilated at the same time by their mutual communication, the dependence of each on one of the electricities of the exterior faces having ceased. But this is not the case: these two electricities being in contact must merely neutralise each other, according to the principles of quiescent electricities, by this contact, as soon as the anterior electricities, having destroyed each other, cease to maintain them separately, They become imperceptible in consequence of this neutralization only, and ought consequently to oppose each other like those of the ribands mentioned above, when we separate the plates again. And this is what experience in fact demonstrates: for, if we attempt to separate the two plates after having discharged them together, we find a resistence as much superior to that displayed by insulating bodies of a thin texture under similar circumstances, as the electricities that concurred to form the charge of the two plates, and which are here converted into quiescent electricities to be revivified by separation, are superior to those that could be imparted to the bodies of a thin texture. And if we overcome this resistance, and actually separate the two plates, the two electricities of the interior faces will resume their intensity, and their tendency to decompose the natural electric state of the surrounding bodies, and in particular of the interior face of the coatings with which the exterior surfaces of the two plates are covered: whence it follows from the known principles, that the exterior faces of these

see what he says on the subject in his *Electricisme artifiziale*. The theory Volta has given of his electrophorus and condenser likewise regards the same subject. I shall have occasion to notice these hereafter.

coatings will give signs of an electricity of the same kind as that of the interior face corresponding to each plate.

One of the uncoated faces of an electrified nonconductor brought into contact with a conductor.

The electrophorus, and doubler.

The phenomena exhibited by a compact, charged, insulating plate, one of the uncoated faces of which is brought into contact with a conducting body, depend on the same principles; but they would also require a minute detail to be treated fully. To this class of phenomena belong the well known effects of Volta's electrophorus: and the same gentleman has freed them to a certain degree from the complication respecting the charge of the insulating body, in his semiconducting plate doubler, the effect of which appears to me, to belong essentially to the phenomenon of quiescent and revivified electricity, exhibited between a perfectly conducting substance, and a substance of sufficient conducting power to exhibit this phenomenon in its simplicity, as incapable of being charged, and yet so bad a conductor as to afford a charge; while, as we have seen, it cannot take place between two perfectly conducting bodies. But I cannot here enter into the particulars, on which the theory of these two instruments depend. I shall only say, that what Volta himself, and since Haüy, have said of it, appears to me essentially to require the principles in question; but as this theory could not be completely developed by these gentlemen, because they had no farther object than to explain the effects of these instruments, they have not generalized it sufficiently. In the course of this paper however, I shall have occasion to touch on some points relating to this subject.

Charged nonconductor.

SECT. IV. Let us now return to the point in question, and apply what has been said of the particular case of the two plates, at which we had stopped, to the inquiry we had in view respecting the state of a charged insulating stratum, or that interposed between the opposite electricities.

Two plates coated on one side, and charged as a single one, exhibit the same appearances, as when charged separately.

For this we want only one more fact, which is equally well established. It is that if, after having charged the pair of plates joined together by their uncoated faces, as if they were a single one, they be discharged in the usual way, and we afterward endeavour to separate the two plates, we observe the phenomena of the revivification of the electricities, similar to those obtained from two plates charged separately, afterward joined together by those faces that have opposite electricities, and discharged in this state as we have already said. This proves, that in the combination of two insulating plates, thus forming but one body, each of the plates takes its own charge; that it to say, there is formed on the lower face of the upper plate an electricity opposite to that communicated to its upper face; that in like manner an electricity is formed on the upper face of the lower plate of the same kind as that communicated to the upper plate; and lastly on the lower surface of the lower plate an electricity opposite to this: and thus the lastmentioned electricity does not correspond directly to the opposite electricity of the upper face of the upper plate, but depends on it only through the medium of the intervening electricities of the two interior faces that are in contact. In fact, since the two plates when separated after their discharge exhibit the same electricities, whether they be charged together or separately, they must be in the same state after the discharge in both cases: but this supposes likewise the same modification in the charged state, since the discharge is made precisely in the same manner, and with the same phenomena, in both cases. It is unquestionably the same, when more than two plates are thus combined; each of them must undergo the same modification as if it had been charged separately, for the number makes no difference here.

The same with any number of plates.

Now as any compact plate may be conceived to be divided into as many strata as there are elementary molecules in its thickness, all these strata must be considered, when the plate is charged, as having each its particular charge, so that the face of one, which is charged with either kind of electricity, is successively in contact with that of another, which is charged with the opposite electricity: for as to the effect in question it can make no difference, whether the strata be simply in contact or adhere together, since in both cases they form but one continuous substance.

One solid plate therefore may be considered as a number of infinitely thin ones.

The following is the idea therefore that facts have led us to form of every insulating stratum charged with electricity, or, which comes to the same thing, taken between two opposite electricities: It ought to be conceived of as formed of an infinite number of strata, all which, however thin they

This the general principle.

are, exhibit on their opposite surfaces electricities of opposite kinds, as well as the assemblage resulting from them.

Coulomb and Haüy infer the same if magnetism and the electricity of the tourmaline.

Coulomb and Haüy have been led to an analogous result in their inquiries concerning magnetism, and the electricity of the tourmaline, but they had not extended this idea to every charged insulating stratum. It appears however, that the modification of the heated tourmaline is not even a particular case of the general principle we have just admitted: that this stone is not then simply a charged insulating body, or a body interposed between two contrary electricities; but exhibits on its surface a modification of electricity, which is perhaps more analogous to the state of a conducting body, the natural electricity of which is decomposed. But this is foreign to our purpose.

This requires some alteration of terms.

The addition that has just been made to our ideas respecting electricity obliges us to a small modification of our nomenclature likewise. Hitherto we have considered an electricity, which is on the surface that serves to limit two different bodies, as belonging to the surface of either indiscriminately. Thus the electricity that is between the interior surface of a coating, and the surface of a plate of glass to which the coating is applied, might equally be called the electricity of the coating, or the electricity of the face of the plate. Yet this electricity may have a different relation to these two surfaces: one may be that of a body, through which the electricity in question supports itself by its attraction for another electricity of the opposite kind, and in the thickness of which it consequently occasions the peculiar modification we have established: while the other of these surfaces may be nothing but the mechanical support as it were of the same electricity, or belong to a body, through which it does not exert the particular action abovementioned. This is precisely the difference between the surface of a plate of glass and that of its coating, or more generally between the surface of the air that surrounds an electrified conducting body and the surface of that body: for we see clearly, that a conducting body cannot have two electricities of a different kind on its surface separated by its thickness alone. It is improper therefore, to give the same name to these two different conditions. To distinguish them without deviating more than is necessary from established custom, I shall say, that a body is *electrified*, or *endued* or *animated* with electricity, when the electricity remains on this surface without occasioning in the body to which it belongs the modification I have spoken of, whether the body be a conductor or an insulato ; and with respect to the surface of the body that undergoes this modification, I shall merely say, that the electricity is applied to it, which can only take place for an insulating body. Thus in a charged plate of glass it is the coatings that I consider as *electrified;* but the electricity of the interior face of each coating is *applied* to the face it coats. It may happen, that an electricity at the limits of two bodies may affect these bodies equally with the modification in question; and then this electricity may be considered at once with respect to each of these bodies as belonging to their contiguous faces, or applied to them.

Charged plate

The name of electric charge will continue to indicate, as it has hitherto done, the state of an insulating stratum interposed between two electricities of opposite kinds, namely, that to the opposite faces of which these electricities are applied, a state which is the subject of the present paper.

What takes place in the discharge.

SECT. V. The facts, that have led us to form an idea of the electrical charge, necessarily give us likewise more accurate ideas of what passes in discharging a charged insulating body. It is clear from what we have seen to take place, in the two plates of glass united, that the discharge only obliges the opposite electricities, which supported each other alternately through each of the strata into which the insulating body might be conceived to be divided, to become *the electricities of the faces of these strata,* to which they were respectively applied before the discharge, and to rest against each other in pairs in a state of perfect repose; namely, that of each face against that of the contiguous face of the next stratum; so that, instead of an infinite number of charged but very thin strata, the result is an infinite number of pairs of electricities neutralized by contact.

The extreme electricities do not destroy each other by the communication.

What has been said in speaking of two plates might lead us to suppose, that this transformation, this different arrangement of electricities by pairs, was the consequence of the extreme electricities of the whole stratum, that is to say, the two electricities of the interior faces of its coatings, mutually destroying each other by communication: but this is not quite accurate. In fact experience teaches us, that there is a revivification of electricity even between one of the faces of a plate and its coating, when we come to take it off after having successively charged and discharged it. Even the fundamental property of the electrophorus of Volta is connected with this phenomenon, if we consider the electricity imprinted on the plate as occasioning a charge there, which is true at least with respect to one part of this electricity; and the disk as a coating, by means of which we destroy this charge in touching it before lifting it up. By this separation the coating or disk is made to exhibit an electricity of the opposite kind to that which it had when it was in contact with the charged insulating plate; and on the contrary the face of the plate exhibits the same kind of electricity, as the coating had before the discharge, and which then consequently was only *applied* to this face. It is clear from this, that the discharge has not actually taken away the electricity, that was applied to this face; and occasioned the charge of the plate; and that it has done nothing more than oblige it, from electricity of the coating, which it was before, to become electricity of the face of the insulating body, and in this quality rest itself against another contrary electricity, which was formed by the discharge of the interior surface of the coating: in the same manner as that of the other strata, which we conceive in the insulating body, rests after the discharge on the electricity that was applied to the face of the contiguous stratum, and becomes the electricity of this face. It is the same with the opposite coating. It is then by the mutual decomposition of the natural electrical state, to which the two coatings are reduced by the transportation of their preceding electricities to the faces of the insulating plate, that the discharge is made: to form these two new electricities, which become quiescent, and are necessary to place in the same state all the other electricities of the insulating stratum; these two electricities of opposite kinds, which each of the two coatings acquires at the expense of the natural state of the other; is the end of that transportation of the fluid, which occasions the shock; and not to destroy

Electrophorus.

In the discharge the electricity from each side is transferred in part to the other, and thus renders it quiescent:

destroy the existing electricities by a communication between them.

and thus the number of electrical srata are increased by two.

Thus, if the preceding considerations have already shown us, that the discharge does not really destroy all electricity in insulating bodies once charged, but merely changes perceptible electricities into quiescent, a result that may appear singular, the last reflections that have been made led us to a result still less expected, namely, that the discharge increases the number of these electricities by two, to render them all imperceptible.

The mode in which electricity acts may be the subject of future inquiry.

SECT. VI. It remains now to inquire, how far these new ideas of electrical charges and discharges, or of the modification assumed by an insulating stratum interposed between two electricities of opposite kinds, may facilitate our investigation of the mode in which electricity acts: but this inquiry, which demands farther preliminary reflections on other points of electrical science, may form the subject of future communications.

VII.

Account of an Experiment in which Potash calcined with Charcoal took Fire on the Addition of Water, and Ammoniacal Gas was produced. In a Letter from JAMES WOODHOUSE, *University of Philadelphia, &c.*

To the EDITOR of the PHILOSOPHICAL JOURNAL.

SIR, *Philadelphia, Sept.* 15th, 1808.

Soot and pearl-ash exposed to an intense heat.

HAVING been engaged in the analysis of soot, I exposed half a pound of this substance in powder, mixed with two ounces of pearlash, in a covered crucible, to the intense heat of an air furnace, for two hours.

When cold took fire on the addition of water.

Not hidrogen but ammonia.

When the mixture became cold, it was emptied upon a plate, and a small quantity of cold water poured upon it, when it immediately caught fire. Expecting there was a decomposition of water, I placed my nose over the mixture, in order to smell the hidrogen gas, which I supposed would be thrown off, but was astonished to find a disengagement of ammoniacal gas.

cal gas evolved.

The experiment was repeated with common charcoal, with exactly the same result.

Charcoal gave the same result.

Azote is one of the component parts of ammonia. Now, as this base is not contained in either potash, water, or charcoal, whence did it arise, to form the ammoniacal gas?

Whence came the nitrogen?

Is it one of the component parts of potash? or is this substance a triple compound, formed of oxigen, azote, and the peculiar metal, which Professor Davy has discovered?

Is it a component part of potash?

Nascent hidrogen sometimes combines with the azotic portion of atmospheric air, and forms ammoniacal gas; but this is not the case in my experiment, for, if the fire of the mixture of charcoal and potash be extinguished by water, and it is then immediately placed under a bell glass containing atmospheric air, the oxigenous part will be absorbed, and the azotic air, will be left behind.

The ammonia not from nascent hidrogen combining with the nitrogen of the atmosphere.

No carbonic acid will be formed.

I am, Sir,

Your humble servant,

JAMES WOODHOUSE.

VIII.

On the Advantages of employing Coal Gas for Lighting small Manufactories, and other Purposes. In a Letter from Mr. B. COOK.

To Mr. NICHOLSON,

SIR,

I Have taken the liberty, from reading in your Journal for September, the paper of Mr. Murdock on the gas light, to address the few following remarks to you. The more the advantage arising from the use of gas is clearly stated, the more generally and simply it is explained, to induce manufacturers and others to make use of it, I think the better; especially now, through the present rupture with Russia and the other northern

It would be beneficial to state the advantage of gas light clearly.

northern powers, the want of importation of tallow has increased to a very considerable height the price of candles, soap, &c. The rise of price in candles has of course been the occasion of an equal rise in oil, as lamps are substituted in the place of candles.

Coal abundant in this country:

This country produces a vast quantity of coal, in almost every part where it is properly sought for, and if the gas light was generally introduced into the greatest part of the large, the middling, and even the smaller sized manufactories, a natural consequence would be, that coal would be consumed in much greater quantities. It might raise the price, but it certainly would be a stimulus to men of landed property, to seek for it, where to the present it has been supposed a stranger. It would therefore, if the demand was so much the greater, be found I am sure in greater quantities than at the present, as miners would be induced to seek it every where. In times like the present, when we are in great measure hindered from exporting it, it would be an advantage if we could consume it all; and in fact at all times, if the whole of the coal produced in this country could be consumed, it would supersede this anxiety for exportation, especially if it brought a little higher price.

and if the consumption raised the price a little, it would occasion a greater supply.

Coak might be more generally used than at present,

In your remarks on the paper, you seem to think, that, was the gas used generally in lighting the streets, and add to it, if generally used in manufactories also, the great quantities of coak produced would be so much more than the demand for it, that it would sell much lower than the present market price. This would certainly be the case, unless coak could be introduced into more general use than it is at present. But from experience I find, that a fire made of coak will last much longer than one made of coal; for, the gas being extracted, it loses that degree of inflammation, which, at the time it blazes, consumes the coal very fast; especially if it is good coal, which contains a large quantity of gas. When I speak of coak used in the place of coal as an advantage, it is in stoves in warehouses and shops, where stoves are in more general use than fire-places. These having a quick draught, the coal, especially as I said before, if good, soon flares away; but if coak is used, that inflammatory principle being taken away, it glows, casts out a great

at least in stoves in shops and warerooms a great heat, and lasts much longer. I may say two fires of coak will last longer than three made of coal, so that I do think that coak, bought at its present price, is equally as cheap, if not cheaper, than coal. Therefore manufactories would experience no difficulty from the increase of coak, as each manufacturer would burn his own coak: it would be only the coak produced from lighting the streets, that would be required to be sent to market, and that only in the winter quarter; and if no coak was made at the coak works, and in fact, the demand on those works would in a great measure be at an end, they would be forced to bring their coals to market.

Coak burns much longer than Coal.

It would put stop to coak works and hence an additional supply of coal.

I do not think then the coak made would be so much above the demand, for it is only in large manufacturing towns, where coaks are used in quantities; and in those towns, if they use the gas, they will make perhaps as much coak as they may want on the spot; they will therefore save all the expense of the carriage of the coak from the works where it is made. Besides, were they to sell it, they could afford it much lower on that account; for, when the coak is made at the works, the gas is all lost, beside the expense attending the making, and the carriage of it to market. It might therefore, if the streets were to be lighted by it, be afforded at a lower price, if it was found that more was made than could be used in the regular way, to people who would burn it in their stoves. It would certainly make a reduction in the profits calculated to arise by lighting the streets, if it was sold at a lower price; but this I do not think would be the case, as the demand, especially in times of good trade, is always great.

The coak would not much exceed the demand,

and therefore the price would not be much lowered.

From the tar I conceive a spirit might be made, as a substitute for the tar spirit brought from Russia, &c., that would be of vast importance to a great number of manufacturers, especially japanners, &c., that article having advanced from perhaps three shillings or three shillings and sixpence to twenty shillings per gallon since the stoppage of trade from the north. If this end could be attained, the tar would always be a source of considerable profit, and make us independant on any other country for a supply of that article.

Tar spirit might be manufactured, instead of that imported.

The

Advantages of the more general use of gas.

The general use of gas would give us several great advantages: first it would prevent the great demand for tallow; and candles would never be so expensive as at the present: secondly it would in part take the place of oil; and thirdly it would render soap lower, as the fat used for candles, might be employed for soap making, saying nothing about the possibility of making spirits from the tar. Besides, if this could not be done, the tar is a very excellent coating for all out-door work, such as gates, fencing, and paling; as well as for boat builders and shipwrights, it being a certain preservative from the worm or the rot in wood exposed to the air, or lying in the water; by coating the articles once in two years well with it. It is infinitely better than the common paint used now: besides if the thing was general, and such quantities of tar were made as would be the case, I should suppose government would recommend its use in the dock yards in order to encourage its consumption in preference to that imported; for it is without doubt superior both as a preventive of decay, and a preservative from the worm in ships' bottoms.

Use of coal tar.

All these advantages we have within ourselves, in that article which abounds in such plenty all over this island—coal.

Utility of the coal gas to small manufacturers.

I now proceed to state the benefit I have derived from the use of the gas in my small manufactory, in order that small tradesmen may make a comparison themselves, and see what an advantage they may derive from its use. Mr. Murdock's paper is on the great scale, therefore far above the calculation of the simple mechanic; and it is to the great number of these that the thing ought to be made clear. To them a small saving of ten pounds per annum is of as great consequence as to the wealthy their thousands.

Apparatus on a small scale described.

My apparatus is simply a small cast iron pot of about eight gallons, with a cast iron cover, which I lute to it with sand. Into this pot I put my coal. I pass the gas through water into the gasometer, or reservoir, which holds about four hundred gallons; and by means of old gun barrels convey it all round my shops. Now from twenty or twenty-five pounds of coal I make perhaps six hundred gallons of gas; for when my reservoir is full, we are forced to burn away the overplus in waste, unless we have work to use it as it is made.

But

Calculation of the profit.

But in general we go on making and using it, so that I cannot tell to fifty or one hundred gallons: and in fact a great deal depends on the coal, some coals making much more than others. These twenty-five pounds of coal put into the retort, and say twenty-five pounds more to heat the retort, which is more than it does take one time with another, but I am willing to say the utmost, are worth fourpence per day. From this fourpence we burn eighteen or twenty lights during the winter season. The candles we used were six to the pound, which on an average one time with another would be about twopence each, though now nearly twopence halfpenny. Say eighteen candles at twopence each are three shillings a day, or eighteen shillings a week; and that each man burnt his candle for twenty weeks only in the year, though for the winter quarter he in general has burnt two instead of one; making the annual amount eighteen pounds.

Utility of the gas for soldering.

Besides, my yearly expense in oil and cotton for soldering was full £30, which is entirely saved, as I now do all my soldering by the gas flame only. My trade is that of a manufacturer of toys, in metal and gold. Now in all button soldering, all the plated articles, in fact all trades in which the blowpipe is used with oil and cotton, the gas flame will be found much superior, both as to quickness, and neatness in the work; for the flame is sharper, and is constantly ready for use, while with oil and cotton the workman is always forced to wait for his lamp getting up; that is, until it is sufficiently on fire to do his work. Thus a great quantity of oil is always burnt away useless; but with the gas, the moment the stop cock is turned, the lamp is ready, and not a moment is lost.

Expenditure for gas.

You see my weekly expenditure in coal does not exceed two shillings; and if I allow five shillings a week to a man, to employ part of his time to attend and make the gas, the expense will then be seven shillings. The yearly expense, if I take it at the same the whole year (although for twenty-five or thirty weeks in the year none will be required as candles) will only amount to £18 4*s.* I have, I know, in the instance of candles, much underrated the expense, as also in oil. I have also estimated the expense in coal, &c., quite high enough; and the coak I find equally as cheap to burn in my small stoves in the shops as coal; so that I do not overrate this, when I say £2 10*s*. for it. The expense of putting up my apparatus was about £50; but not knowing the cheapest and readiest methods to go about it, it cost me more than it ought by £15. I will say £40, for which in my statement at the conclusion I shall allow interest.

Advantageous on a still smaller scale.

If erected on a smaller scale, the saving to the manufacturer is equally as great; for the poor man, who lights only six candles, or uses one lamp, if the apparatus is put up in the cheapest way, will find it cost him only £10 or £12; which he will nearly if not quite save the first year. And if the pipes are made of old gun barrels, as mine are, and once a year, or once in two years, are coated over with the tar to keep them from rusting, they will last half a century. The burst or waste barrels, that used to be sold for old iron, would then produce a better price to the gun makers; and the pipes would be better and more durable, than if made of more slight materials.

The profit underrated.

You see I have reckoned the five shillings per week for the man the whole year, as also the same expense for coal for the whole year, of course that is reckoning more than I ought by nearly a fourth; but where soldering with the blowpipe is necessary, gas will be wanting, although in smaller quantities, in summer as well as winter, and I am desirous of overrating the expense, rather than otherwise, for fear of any accident in a retort being melted, &c.

Now I do think, the more generally this is made known, that the industrious tradesman may derive from it the benefit he ought, the benefit nature has so bountifully supplied this nation with, the better: especially when candles and oil are risen to so great a price, which is a very great drawback on his profits and industry.

If you can extract any thing from the above imperfect description, that may be of any use, and put it into a shape, so as to make it worth inserting in your Journal, I should be glad. You see what my object is: to show to the middling man as well as the great one, that a considerable saving and advantage may be derived from the use of gas in his manufactory. I have said nothing about the greater safety there is in its use than in that of candles; there being no danger to be dreaded from snuffs and sparks: a circumstance from which I should think the Insurance Offices would be great advocates for its introduction also.

Drawing and description of an apparatus promised.

I will also, if you think proper, send you a plain description of a small apparatus sufficiently and easily explained, that shall enable any man to put it up himself; for the thing is so simple, that with a few plain drawings and explanations almost any man of common abilities may do it. It is often the case, that things of great advantage and use to the community at large are kept back and as secret as possible by individuals, who have had the good fortune to derive much advantage from them: but if any thing useful can be introduced for the benefit of mankind, that man is deserving of thanks, who uses the best means nature has bestowed upon him to disseminate its usefulness aboad.

Dr.			*Cr.*		
Yearly expense in coals and man	£18	4	Twenty weeks at eighteen shillings per week for candles	£18	0
Interest on Forty Pounds......	2	0	Oil and cotton for lamps	30	0
Profits per year..	30	6	Coaks worth	2	10
	£50	10		£50	10

I reckon nothing for the tar, setting it against any little loss or accidents.

I am, Sir,
Your humble servant,
B. COOK.

Caroline Street,
Birmingham, Nov. 22d, 1808.

REPLY.

I Shall with great pleasure receive and attend to the drawing and description offered by Mr. Cook, whose clear descriptions of matters of fact possess a value, which needs not the addition of my suffrage to recommend them.

W. N.

IX.

IX.

Extract from a Letter to J. C. Delametherie, *on Volcanic Substances, by* Lewis Cordier, *Mine Engineer*[*].

Work on volcanic substances promised.

I Have again visited the mountains of Auvergne, and have finished certain observations and experiments, which will enable me to present the public with a work on different volcanic productions. The following are some of their results.

Titanium in volcanic sands

" All the ferruginous sands of volcanoes capable of being attracted by the magnet are composed of oxide of iron, and oxide of titanium[†].

and most lavas.

" The major part of lavas contain a perceptible portion of oxide of titanium.

Granitoid lavas.

Summit of Meisner.

Probably volcanic.

" The porous or massive granitoid lavas of the extinct volcanoes in the exterior of France are composed of feldspar, pyroxene, and titanized iron." On comparing them with the green granitello which is found on the summit of Meisner in Germany, and which Werner places in the first rank of those rocks that he calls secondary greenstone, they appear to be perfectly similar. It is no doubt difficult to conceive, how all the authors who have written on the granitello of Meisner could deceive themselves respecting its composition: and such a mistake is so much the more surprising as this rock has given rise to various commentaries. It is certain however, that it is not formed of feldspar and amphibole, as has hitherto been supposed, but of feldspar, pyroxene, and titanized iron, which are very different. This discovery adds fresh weight to the opinion advanced by Mr. Voigt and several German mineralogists respecting the Meisner. It is extremely probable, that the summit of this mountain is in reality a fragment of volcanic strata.

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[*] Journal de Physique, vol. LXIII, p. 235.

[†] We must except those sands however, the base of which is specular iron ore; but these are extremely rare.

X.

Letter to J. C. DELAMETHERIE *on some Granatoid Lavas, by* J. P. D'AUBUISSON[*].

Some granitoid lavas composed of feldspar and hornblende.

I Have read with much pleasure in your number for September last the letter of Mr. Cordier, in which this mineralogist communicates to you the principal results of his observations and experiments on volcanic products[*]. To his third assertion, " the porous or massive granitoid lavas of the extinct volcanoes in the interior of France are composed of feldspar, pyroxene (augite), and titanized iron," I can add, " some of the lavas too are composed of amphibole and feldspar."

Lava from Cantal

a true secondary green stone.

I have in my possession a specimen of lava from Cantal, which is composed of, 1st, amphibole in long crystals, very black, perfectly laminar, and exhibiting in the most distinct manner the two directions of the laminæ, cutting each other at an angle of about 124°, which, as is well known is the distinguishing characteristic of the amphibole: 2dly, feldspar in crystals of a vitreous aspect, like that of almost all volcanic products: 3dly, a blackish gray matter perforated with numerous small pores. This matter predominates in the mass; yet in some places the amphibole is more abundant. This lava is a true secondary greenstone; that is to say, one of those found in the secondary trapformation, and which are composed chiefly of amphibole and feldspar.

Passes into a basalt composed of its elementary molecule.

If the crystals that constitute the lava of which I speak diminished in size so as to be no longer distinguishable by the eye, and the whole ultimately formed a homogeneous mass, which certainly happens in various parts of the stream from which the specimen I possess was broken off, the result would be a compact black rock, a real basaltes, composed only of the elements of amphibole and feldspar, the same *greenstone* but in a compact state: it would be to it nearly the same as compact limestone is to granular.

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[*] Journal de Physique, vol. LXIII, p. 385.

[†] See the preceding article.

Granitoid of Meisner.

The examination of various greenstones of the basaltic mountains of Germany, of the granitoid of mount Meisner among others, had formerly led me to a similar conclusion, and I perceive with satisfaction, that certain greenstones of Cantal indicate a similar formation. This however does not prove, that there are no basaltes composed of the elements of feldspar and augite confusedly united, much in the same manner as we see certain porphyries with base of petrosilex are nothing but compact sienites, or formed of feldspar and amphibole; while others are compact granites, or formed of the elements of feldspar, quartz, and mica.

Augite formerly confounded with amphibole.

The author of the letter finds it difficult to conceive, how all those, who have written on mount Meisner, should have been deceived respecting its composition. I will show how we may readily account for this. Formerly the augite was considered merely as a variety of amphibole (hornblende). Werner was the first, at least in Germany, who distinguished these two substances, which in many respects resemble each other; but he did not distinguish them till after he had written on mount Meisner, and said, that the granitoid on the summit of that mountain was composed of feldspar and amphibole. Authors have since repeated this assertion, and continued to give the name of amphibole to what is in reality augite. I have pointed out a mistake of this kind some years ago; and I have lately mentioned, that part of what some persons, myself among others, had taken for amphibole, in mount Meisner, was partly feldspar coloured green, and partly augite; but I did not go so far as to assert, that this rock contains no amphibole.

XI.

An Examination of a Stone of the Calcareous Species called "Thunder Pick." By Mr. J. Acton, *of Ipswich. Communicated by the Author.*

Common tones too auch neglected.

THE great eagerness, with which newly discovered and rare minerals have been sought after by men of science in order to their analysis, has occasioned the more common ones to be in some measure neglected; so that it is not unusual for persons, who understand the composition of the diamond or other valuable gems, to walk in their fields, and pick up many stones, the nature and use of which they are unacquainted with, though perhaps in a friable state composing considerable part of the soil of which they are the proprietors. And I believe in cases where the more common minerals have formerly undergone examination, such is the present improved state of chemistry, and the consequent greater number and purity of tests and reagents, that it will scarcely be deemed superfluous, to subject them to fresh investigation; particularly if it be done with an ardent view to inquiquiry, and with diligent care and attention to the results.

Even those that have been analysed formerly should be examined afresh.

Thunder pick described.

Influenced by these considerations, I have ventured upon an analysis of a stone of the calcareous species, frequently met with in this country, and called by the common people thunder pick, from the supposition of its falling from the clouds in storms of thunder and lightning. It occurs in crystals weighing from 10 to 1000 grains, of a conic shape, with a cavity at the base extending about a fourth part down the centre of the crystal. Its colour varies from gray, brown, brownish red, to almost black, semitransparent. The nearer they approach to the red colour, the greater is their transparency. I cannot find they abound in any particular place, but are generally discovered solitary by the husbandmen when at plough, or turning up the earth by ditching or otherwise. When scratched with a knife it has a strong alliaceous or urinous smell. Its cross fracture is fibrous, with the striæ diverging nearly as from a common centre. Its longitudinal fracture is glittering, with the striæ parallel. It is moderately hard, and of the specific gravity of 2·663.

Crystals.

Colour.

Generally solitary.

Smell.

Fracture.

Spec. gravity.

a. When heated upon charcoal before the blow-pipe, its colour disappears, but it is infusible.

Infusible alone.

b. With phosphate of soda it is difficultly soluble and fuses into an enamelled bead.

Fusible with phosphate of soda,

c. With borate of soda it dissolves more readily, and fuses into a semitranslucid white globule.

with borax,

d. With caustic soda I could only partially fuse it into a white enamel.

and partially with soda.

Exp.

Action of heat on it.

Exp. 1. A. One hundred grains of thunder-pick in coarse fragments exposed in a platina crucible two hours and a half to a moderate heat lost only four grains, but afterwards exposed to a much higher temperature for an hour lost 42·90 grains.

B. One hundred grains in one piece exposed in a porcelain crucible two hours to nearly a white heat lost 45·90 grains.

A and B. The residue of these two operations, amounting to 112·20 grains, were exposed in a porcelain crucible for four hours more to an intense white heat. When the crucible was taken out and examined, only 102 grains could be collected, as the remainder had united to the crucible, but from its apparent quantity no loss of any consequence could have been sustained. The crucible as well as its Wedgwood cover had suffered a commencement of fusion, and they could not be separated without breaking.

Dissolved in nitric acid.

Exp. 2. A. Wishing to ascertain nearly the quantity of nitric acid requisite to dissolve a certain quantity of thunder-pick, I weighed 100 grains of it in fragments, and introduced to it 100 grains of pure nitric acid of the specific gravity 1·431, and added more acid by ten grains at a time, till the whole was dissolved. Having thus found the quantity of acid necessary to dissolve 100 grains of thunder-pick, I placed it on the balance, and equipoised it on the other scale; 100 grains of thunder-pick were then conveyed into the acid, and the weight of the carbonic acid gas was found to be 42·40 grains.

In muriatic.

B. I repeated the above experiment, substituting a quantity of muriatic acid of the specific gravity of 1·149 with 100 grains of thunder-pick, and nicely adjusting the balance as before, found the weight of the carbonic acid gas given out to be 43 grains.

Oxide of iron & manganese.

C. The nitric solution (A) being now filtered became nearly colourless, and left on the filter the colouring matter of the thunder-pick. I believe a little oxide of iron and manganese, which, when dried, weighed 0·40 of a grain.

Carbonate of lime.

D. The filtered solution being treated with carbonate of potash, carbonate of lime fell down, which when collected and ignited in a crucible weighed 96 grains.

E. To

. To the again filtered liquor was afterwards added tic ammonia, when no precipitation ensued; but on ting again with carbonate of potash, it occasioned a diness, which fell down, and when collected and ig- d weighed 1·50 grains, which dissolved with effervescence itric acid.

Carbonic acid gas collected.

xp. 3. A. To be still further assured of the above ex- ments being managed in nearly an accurate manner, I 100 grains of thunder-pick into a small gas bottle, and red on a sufficient quantity of nitric acid to dissolve the le. I collected the extricated gas with the mercurial umatic apparatus in nicely graduated jars, amounting 91 cubic inches at the temperature 48° and pressure 8, which by the usual following calculation gave 92·63 ic inches at the medium temperature of the air at 60°, barometric pressure 30 inches.

e of expansion for every degree of the thermometer, according to Gay Lussac.

```
                              60 mean temp.
480)91·00(0·189              48 actual temp.
    480      12              —
   ————    ————              12 difference.
   4300    2·268             —
   3840
   ————
    4600
    4320
    ————
     280
    ————                  91
                           2·26
                         ————
 30 ——— 29·80 ———        93·26
                         29·80
                         ——————
                         746080
                         83934
                        18652
                      ——————————
                  3|0)2779·1480
                      ——————————
                        92·63 cubic inches at the corrected
                        ————      temperature and pressure.
```

If

Medium estimate of the carbonic acid.

If the quantities of carbonic acid gas produced in the foregoing experiments be added together, and the average taken as follows:

42·90 45·90	Extricated by heat.
42·40 43·00	Extricated by nitric and muriatic acids.
4)174·20	
43·55	Average.

And if it be admitted, according to the late accurately conducted experiments of Messrs. Allen and Pepys, that 100 cubic inches of carbonic acid gas weigh 47·26 grains, then by the following calculation 43·55 grains will amount to 92·14 cubic inches, which is within half a cubic inch of the gas actually produced—

47·26——100——43·55   92·63 Gas collected.
100
47·26)435500(92·14 } Cubic inches by calculation.
42534   ——
——— ··49 Diff.
10160 ——
9452
———
7080
4726
———
23540
18894
———
4646

The carbonic acid gas collected in the last experiment being submitted to the test of limewater, by means of Pepys' Eudiometer, from 98 to 99 parts out of 100 were absorbed.

The following therefore appears to be the result of the above analysis of 100 grains of thunder-pick:

Average of experiments produced—

Carbonic acid gas	43·55	forming 97·50 grs. carbonate of lime.
Lime	53·95	
Oxides of manganese and iron	·40	
Water and loss	2·10	
	100·00	

No alumine.

On adding to the filtered nitric solution of the second experiment succinate of ammonia, as a test for alumine, no cloudiness ensued.

Neither was any effect produced on the addition of prussiate of potash for iron.

I had dissolved 200 grains of the thunder-pick in nitric acid, in order to precipitate it, to say how far it would corroberate the former statements, but being interrupted, I unwarily added the alcali without previous filtering, and afterwards a small portion of it; however I proceeded in collecting it, and the precipitate, after ignition, weighed 192·70 grains, which is certainly a nearer approximation than I could have expected under the circumstances.

XII.

Remarks on a Review of Professor Vince's Essay on Gravitation. In a Letter from the Author.

To Mr. NICHOLSON.

SIR,

Remarks on the Edinburgh review of the Essay on Gravitation.

I Shall esteem it a favour if you will insert the following remarks upon the Edinburgh Review (vol. XXV) of my *Essay on Gravitation.*

The fluxion of the elasticity from the change of distance $\dot{x}$ is no solution of the problem I proposed to answer, i. e. "to find the effect of the fluid to impel a spherical body of "infinite magnitude, towards the sun." The reviewer has investigated a point, which has nothing to do with the proposed inquiry; and so little was he acquainted with the subject

subject, that he considers the *accelerative* and *moving* forces of a body as the same thing.

I am, Sir,
Your humble servant,
Cambridge, 24*th Nov.* 1808. S. VINCE.

Some of the hypotheses which have been invented to account for gravitation are so fraught with absurdity, that it was not thought necessary even to state them.

XIII.

An Essay on the Sugar of Grapes; *by* PROFESSOR PROUST*.

Grapes contain a distinct species of sugar.

THE grape presents us with a sugar of a new species, the existence of which had hitherto been suspected only in conjunction with those sweet and agreeable substances, that are known to form the basis of the flavour of our fruits. Before I proceed to the examination of this product, the mode of extracting it, its qualities, and the uses to which it may be applied, it will be proper to lay down some general principles respecting sugar. We must first distinguish its species, take a view of the substances that usually accompany it, and examine which of the latter it is essentially necessary to separate from it, in order to apply it to our use, and which may be suffered to remain without any sensible diminution of its qualities. This is the order I shall pursue in my inquiry; and a concise view of these particulars I trust will be sufficient, to enable us to judge whether the species of sugar I announce have all the characters of the genus; and whether, while it possesses the qualities of being wholesome and agreeable to the taste, it be sufficiently abundant to supply our wants.

Order of the inquiry.

On sugar and its species.

The immediate products of vegetables.

Nature, while she deposits in the various parts of the vegetable structure those compounds, to which we give the name of immediate products, frequently modifies them by slight shades, and varies each in so many different species. Thus starch, gum, resin, oil, tannin, extractive, &c., while they retain the principal characters of the genus, to which they give their names, differ in certain respects, and thus give rise to the species which analysis has discovered.

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* Abridged from the Journal de Physique, vol. LXIII, p. 257.

getables variously modified.

Sugar too has its species, and of these I purpose first to speak, as the ideas with which they will furnish us are necessary to understand what is to be said respecting the sugar of grapes. If we compare these species with respect to hardness or consistency, we shall find a striking difference in this respect. We see, that the product of the sugar cane is dry, brittle, and easily crystallized; while the driest manna softens with a slight heat, and sticks to the finger that presses it. We find too, that the syrupy product which we call mucoso-saccarine is a third species, differing from the former in uniting the viscosity of a mucilage to the property of retaining a softness that no drying can destroy.

Sugar has its species. These differ in consistency.

The honey, that bees collect from plants, and in which it is impossible not to recognise one of their immediate products, will give us an instance of two species combined. That it frequently varies in consistence is well known; and it has been long presumed, that it must contain a portion of crystallizable sugar, which has even been affirmed, though never proved: but as this conjecture has been confirmed by the experiments I have lately made, I shall proceed to relate them.

Honey a compound of two species.

The honey collected at Madrid on the heights of Flonda is yellow. It has the transparency and tenacity of a turpentine to such a degree, that we may justly say it is to solid sugar the same thing as a balsam is to a resin. Alcohol dissolves it almost entirely: a few particles of wax separate from it, and it afterward deposits a small portion of a viscous substance, which is soluble in water, precipitable by alcohol, and without any particular taste. This is a true gum. The white honeys, of which I shall soon speak, likewise contain a little.

Yellow honey.

The colour of the former is certainly owing to an extractive principle, which cannot differ much from that of vegetables, for the muriate of tin precipitates it in a yellowish

Owes its colour to extractive.

lake, while with white honey this muriate scarcely gives any appearance.

Does not crystallize.

The alcoholic solution of this honey left to evaporate spontaneously shows no disposition to afford crystals like those I shall soon mention. Perhaps it contains a little solid sugar, which the liquid sugar retains so strongly as to prevent its separation: but this does not prevent me from considering this honey as wholly, or nearly so, one of the two species of sugar, which I purpose to point out in honeys in general.

Thick honey separates into two portions.

White honey contains most candy.

When a honey is very consistent and opake, we find that in time it separates into two parts: one granular, crystalline, and opake, that collects at the bottom of the vessel; while the other, transparent and fluid without the addition of foreign moisture, remains at the top. We find too, that white honeys are most liable to this kind of separation, or that they contain habitually more candy than the yellow.

White honey treated with alcohol.

A little wax separated.

Crystallized.

Presuming, that, though both the species contained in white honey were soluble in alcohol, that which is fluid would be less so than the other. I added alcohol to some white honey of the finest quality from the mountains of Moya. The result of this operation, conducted with some precautions which may easily be supposed, produced the separation of a white powder, which subsisted spotaneously. This powder, separated from the solution and slightly washed with alcohol, ultimately afforded me a powdery sugar, which I left to dry in a moderate temperature. Nothing more remained, but to purify it afresh, to make it into a sirup, and dispose it to crystallize. Its solution in water occasioned the separation of some particles of wax; after which, having boiled it down to the consistency of a thick sirup, I set it by covered with a paper merely. In less than two days, which I scarcely expected, it began to cover the sides of the vessel with white points, whence I judged at once, that I must not expect a crop of common sugar. In fact on the fourth day the sirup was converted almost entirely into granular, hollow crusts, which had risen more than an inch above its level. These I set by a few days to drain, in order that its melasses might be separated as much as possible. The following are the qualities of this new kind of sugar.

Qualities of the crystals.

It has a considerable resemblance to the head of a cauliflower, is perfectly white, and does not attract moisture. Its sweet, agreeable, and cool taste is less saccharine than that of common sugar, has no resemblance to the flavour of honey, but it leaves on the tongue something I can't describe of farinaceous. It is easy to conceive, that if it were employed to sweeten any thing, more would be requisite than of honey or common sugar.

If it be burned it diffuses the smell and usual fumes of burned sugar. Alcohol dissolves it without any residuum, and by evaporation it separates afresh into granular concretions. Lastly nitric acid converts it readily into oxalic. The melasses that drains from it is nothing but the second kind of honey, of which I shall speak, mixed with a little gum, which alcohol instantly demonstrates.

The fluid portion.

This not free from solid parts.

The second kind however must not be considered as perfectly free from common honey, because the solubility of the latter in common honey and in alcohol are two causes, that prevent obtaining an accurate separation of them. We may succeed better by leaving a solution of honey in alcohol to evaporate in the open air, for then the first crystallizes, and leaves the second tolerably pure. The honey of the mountains of Moya for instance, which is of a superior quality, affords in this way thirty-nine or forty per cent of crystals, while by washing in alcohol we separate only five or six and twenty.

Qualites of the fluid portion.

The fluid honey obtained in this manner is a sugar, that retains a perfect transparency; and however long we boil it, its appearance will only be that of a thick turpentine. It attracts moisture, and is the second part of the sugar, which formed with the first the honey that has just been examined.

I have not examined their proportion in other sorts of honey, for want of time; but till this inquiry is extended to more, we may deduce from these facts some useful inferences respecting the nature of sugar.

General properties.

In the first place they show us, that the sugar collected from flowers by the bees is of two species. They show us too, that these species, united in honey, and compared with the sugar we derive from plants, resemble it likewise in two points worthy of remark. The first consists in the two degrees of consistency; the one solid, the other soft, which in like manner divide all the vegetable sugars: the second in the flavour, which is commonly more sweet or saccharine in the fluid honeys and sugars, than in those which are crystallizable.

The solid portions not common sugar.

The solid sugar of honey is not similar to that of the cane, either in flavour or crystallization: but in both these qualities it comes so near the sugar of the grape, that I begin to doubt whether there be much difference between them. Unquestionably it would be an important advantage to society, to be able to separate the two species of sugar, that compose most kinds of honey, in order that each might be employed exclusively for those purposes, to which it is best adapted: and though at present I see no hope of succeeding in this but by the mean of spirits of wine, which would be far from economical, I cannot avoid thinking, that the result would be one step toward the emancipation, for which a great part of Europe is anxious, if the sugar of grapes did not offer itself, to hasten a period so desirable.

Their separation difficult,

but the grape may render us independent of the West Indies.

Manna.

Its softness not owing to extractive.

It has long been supposed, that the softness of manna, and the readiness with which it grows moist, were owing to an extractive matter; and that this matter, covering the qualities in which it resembles sugar, must be the cause of its laxative virtue. If however we examine its solution with muriate of tin, we find but very little precipitate: and alcohol dissolves manna completely, contrary to the opinion of Lemeri. This solution, left exposed to the air, dries into a porous mass composed of very slender crystalline filaments and granular particles, which by its lightness resembles a fine white agaric.

It is a distinct species of sugar.

Manna thus refined does not approach the sugar of the cane: its moistness and faint taste are still the same. It is not in its nature therefore, to be any thing but what it has always appeared to us, that is to say, a species of sugar, the characteristics of which are softness, an unpleasant taste, and the medicinal properties for which it is used. To ascertain whether manna likewise have its two species, and be in this respect analogous to honey and other sugars, it would be necessary to analyse some fat mannas, of the purity of which we could be certain; but this is not at present in my power.

Yields oxalic and mucous acids, but honey does not:

A distinguishing character of manna is to form with nitric acid the two acids afforded by gum, sugar of milk, mucilage of linseed, &c.; while honey, which approaches manna in degree of consistency, does not.

Abundant in America,

and in Spain.

Manna abounds in America, according to the report of travellers. Herrera says: "In the season there falls a large quantity of a dew, which coagulates like sugar, and the use of which is so wholesome, that they call it *manna*." Is this our manna? or is it a particular kind of sugar? Father Picolo, one of the first spiritual conquerors of California, likewise asserts, that it exudes from the shrubs abundantly in April, May, and June. In Spain manna is so plentiful, that all Europe might be furnished with it, according to the report of two members of the Academy of Physic at Madrid, who were directed to make the inquiry by the Marquis of Ensenada.

Sugar exists in many plants.

There is at present no doubt, that sugar exists in a multitude of vegetables, fruits, roots, and stalks, in the sap of the palm, birch, maple, bamboo, maize, &c.: but we do not yet know, whether that of the beet, from which Achard has professed to manufacture it, and of other vegetables, in which Margraff discovered it, be really of the same quality as that of the cane, or a different species; like those that follow.

Maple sugar.

It does not appear for instance, that the sugar of the maple is very similar to that of the cane. The juice of this tree commonly affords five per cent of solid sugar: it is to be presumed, that it has likewise its melasses, or sugar of the second species. Travellers say, that it is three or four times as long in dissolving as the sugar of the cane; that it sweetens less; that the latter is preferred to it for chocolate; and that a portion of the latter is mixed with it for confectionary. Hence it should seem, that the sugar of the maple is not so agreeable as that of the cane.

Sugar from the locust tree or St. John's bread.

We are told too, that in Egypt they extract from the pod of the carob tree a kind of honey, which is much prized by

the

Vinous and spirituous liquors from it.

the Arabs. I know already, that this sugar is not crystallizable, but of the second species; and it contains an extractive matter, which gives it a high colour, and spoils it by a particular flavour, which assuredly would not be relished by the rudest Bedoween in Europe. Its wine much resembles that of melasses, and where no other was to be had, might be drunk without repugnance. It is very intoxicating. The brandy produced from it I have made known already.

Sugar in various fruits.

A sugar equally crystallizable with that of the cane, but very different from it, exists in the gooseberry, the cherry, and the apricot, the juice of all grapes, and no doubt many other fruits. Its crystals are pulverulent, and so difficult to perceive, that I have not yet been able to observe their shape. This produces the concretions, that are found in raisins.

Sugar of figs,

Figs appear to contain a great deal of crystallizable sugar, since I am informed, that thick crusts of it separate from them in the barrels in which they are kept dry.

of gooseberries and cherries,

The candy that forms among preserved gooseberries and cherries equally belongs to these fruits, and not to the sugar of the cane. These concretions dissolved in alcohol always resume the granular form, which is commonly found in these preserves.

of the apple, quince, medlar, plum, peach, &c.

The first species of sugar does not appear to be formed in the apple, quince, or medlar. Their juices afforded me only the second loaded with gum and extractive colouring matter. It is probably the same with plums, peaches, &c., for candy is scarcely ever found in their jellies or preserves. In all these fruits however the sacharine product is confounded with gum, extractive matter, the malic, citric, and tartarous acids, sulphate of lime, &c.

The two species variously distributed among fruits.

These facts, which deserve a farther examination in the vegetable kingdom, contribute still more to confirm the existence of the solid and mucoso-saccharine species of sugar which appear to divide our fruits in very various proportions.

The fluid species.

The fluid sugar, which had received the compound name of mucoso-saccharine, because it was considered as a mixture of solid sugar and mucilages, was not well understood by any one before Deyeux: He perceived, that it was a species of the genus, habitually fluid, and to be classed among the immediate products. He judged too with reason, that of the two species of sugar known it was the only one capable of fermenting by itself, while the other will not undergo this change unless disposed to it by some ferment.

The only one that will ferment spontaneously.

The labours of Duthrone too has so clearly confirmed the existence of this product by the various facts he has collected in his work, as to render it no longer disputable, that the only object of the sugar-makers must be, to separate the crystallizable sugar from the fluid: but I shall give here the results of the analysis I have begun of the sugar-canes of Malaga.

To separate this from the solid the only object of the sugar maker.

In the juice of these, when fresh drawn, we find green fecula, gum, extract, malic acid, sulphate of lime, and the two species of sugar. All these products, except as to their varieties, are the same as those met with in most fruits.

Juice of Spanish sugar canes examined.

A slice of the cane put into infusion of litmus reddens it powerfully: yet its juice is not perceptibly acid to the taste, because the acid in it is only in a very small quantity, and the sugar covers it; but in the juice boiled down it is plainly perceivable. The following are the effects of reagents on it.

Contains an acid,

The oxalic acid and barytes form a copious precipitate with it, which proves, that it contains sulphate of lime. Concentrated solution of platina throws down nothing so that it contains no salt with base of potash.

sulphate of lime,

no potash,

Alcohol poured on the sirup of the cane separates from it insoluble filaments, which fall to the bottom of the vessel, and are pure gum. Some time after the gum a small portion of a white powder falls down, which is sulphate of lime. This single product classes the juice of the cane with that of most fruits.

gum,

The sirup, when freed from the gum and sulphate, forms copious precipitates with the nitrates of lead and silver. With limewater too a precipitate is formed, and the liquor turned green. This indicates the presence of extract, which muriate of tin confirms by precipitating a whitish lake. The

extractive matter.

same sirup distilled with weak sulphuric acid affords no signs of vinegar; the acid it contains therefore is not volatile.

and malic acid.

The use of repeatedly adding potash and lime in purifying sugar,

not ascertained.

If this sirup be boiled with chalk, its acid is saturated; and from the filtered and concentrated juice alcohol separates malate of lime, but in so small a quantity, that we need not be surprised if Macquer and Darget, in the experiments they made at Bercy near Paris, did not perceive any acid in the juice of the cane. Duthrone, when he said, that the repeated employment of potash and lime in the clarification of sugar must have for its object, to saturate any thing but acids, was in the right. He even thinks, that the alkalis combine with some remains of glutinous feculæ, and thus lessen their too great solubility. Yet as it seems difficult to conceive, according to our ideas of the properties of gluten, how it can remain dissolved in so large quantity in juices void of acid, or deprived of it by the first saturation, I dare not at present adopt this opinion, because I do not see clearly what is the use of the alkalis in the clarification of the juice of the sugar-cane.

Infusion of the fresh cane

yields ammonia.

The cane cut into thin slices gives out its soluble parts to water. On concentrating this solution, a little before it boils a greenish, feculent film separates, which does not differ from that of gooseberries, grapes, &c., and affords abundance of ammonia on distillation. Duthrone obtained the same result.

Boiled down to muscovado.

Its flavour.

If it be boiled down to the consistence of a thick sirup, in fifteen or twenty days a congelation like honey will be produced, sufficiently firm to remain fast in the vessel. The taste of this muscovado sugar is pleasant; and it has an aromatic flavour, which may be better recognized in melasses, or still more in rum. The race of this liquor therefore is really that of the cane: it is a product of the plant, and not a precipitate formed in the preparation, or by any of the changes to which the juice is exposed before it arrives at the state of melasses.

The cane affords half its weight of juice.

According to Duthrone, the cane commonly affords half its weight of juice. This juice marks on Baumé's areometer from 5° to 14°, a difference depending on the ripeness, and the influence of other causes, which occasion an increase

or

diminution of the products of the cane, as of other ats. According to him 14° indicate twenty-five pounds ven ounces of sugar to a hundred pounds of juice; l, as in the most favourable circumstances the cane does yield above half its weight of juice, a hundred pounds the cane cannot produce more than thirteen of raw su- . If we speak of refined sugar, this product must be luced at least one third; since raw sugar appears to con- n not much less than this proportion of melasses. The portion of dry to liquid sugar however is yet to be ascer- ned. No doubt it will vary according to the strength of plants, but it deserves to be inquired into, and I shall end to it when I resume my examination of the canes of laga. To return to the muscovado, or raw sugar,

and at most 13 per cent of sugar, or about eight of refined.

Muscovado long in use in the East without refining.

When we consider this honeylike mass, such as it is afford- by the evaporation of the juice, that is with its sweet and reeable taste modified by the slight bitterness of its ex- ctive principle, we may reasonably conjecture, that the ental nations, after they had discovered it, and placed it ong the condiments adapted for seasoning their insipid e, would employ it for many centuries in this state, they did honey; and we may presume it was from the re- mblance of honey to the raw sugar then in use, and not to fined sugar, that some of the ancient naturalists termed gar "another kind of honey, that is formed in reeds." oney itself, the only production that has any real resem- ance to muscovado, not being capable of any process of finement to improve its qualities, they would naturally ntinue long of the opinion, that raw sugar was equally in- sceptible of that degree of perfection, to which it has een brought in modern times: and if we consider the mber of ages that elapsed, from the time when corn be- n to be the general food of man, to that in which he dis- vered the art of making fermented bread, we shall find is conjecture respecting raw sugar extremely probable. esides, it is proved by the historical researches of Du- rone, that toward the end of the fourteenth century raw gar, without any farther purification, was an article of ade in Egypt, Syria, Cyprus, &c.

But if the refinement of the honey of the cane have hap-

Part of the

pily

sugar is lost by refining.

pily enabled us to enjoy the use of sugar in all its purity, we must confess, that we do not obtain this advantage without sacrificing a part of the saccarine matter it contains: for it is certain, that, if the melasses, which probably amount to more than a third, could likewise be deprived of the extractive matter concentrated in it by evaporation, as well as by the several preparations it undergoes, with the foreign matters introduced into it by the potash, lime, and bullock's blood, we should have in it a sirup, which, notwithstanding the inconvenience of its fluidity, would be a very useful substitute for sugar, in all cases where the luxury of our tables does not render the latter indispensible. And it would have the farther advantage of sweetening in smaller quantities: at least I may reasonably infer this from the melasses I have separated from raw sugar, the qualities of which render it far superior to the melasses of our sugar refiners, as it is not contaminated by any foreign mixture.

(To be concluded in our next.)

SCIENTIFIC NEWS.

An Essay on the Warming of Mills, and other Buildings, by Steam. By ROBERT BUCHANAN, *civil Engineer. Glasgow.*

Buchanan on warming buildings by steam.

IN this little but valuable pamphlet Mr. Buchanan has collected the principal facts relative to the application of steam for the purpose of communicating heat. There are two points of view in which this subject may be considered, safety and economy. In large manufactories of combustible articles the safety arising from the exclusion of coal fires must be an obvious advantage. How far it may be economical must depend greatly on local circumstances.

History.

The idea was suggested by Colonel Wm. Cook, in the Philosophical Transactions for 1745, but it does not appear to have been applied practically. Mr. Snodgrass first applied it to the warming of cotton works in 1798; see our Journal, vol. XVI, p. 326: and his example was followed by others.

Heads of the subject.

Mr. B. arranges his subject under the following heads: 1. The proportionate size of boilers and quantity of fuel. 2. The

he proportion of steampipe required to heat a given
e. 3. The substance and colour of steampipes. 4.
ir direction and arrangement. 5. The modes of con-
ing them.

Size of boilers and quantity of fuel.

A cubic foot of boiler will heat about 2000 cubic feet
pace in a cotton mill, where the temperature is in ge-
l from 70° to 80° of Fahrenheit. The boiler is supposed
e of the shape commonly used for a steam engine, and
ubic feet to be equal to a horse's power. Where the
er is separate, and not used for the joint purpose of
ing an engine and warming a building, it should be
iderably larger than in this proportion, to avoid the in-
lity of heat incident to a boiler working at its full capa-
: 25 cubic feet of boiler require about 14lbs. of good
castle coal per hour.

Proportion of pipe to space.

. Every square foot of exterior surface of steampipe
warm about 200 cubic feet of space in a cotton mill. A
ll chapel has been warmed comfortably with half that
portion. The safety valve in the boiler is supposed to
oaded about 2½ lbs. to the square inch. If the steam
e stronger, it would give more heat, but it would be
cult to keep the joints of the pipe steam tight.

Materials of pipes.

. Cast iron pipes give out above twice as much heat as
per, or tin, unless the tin be painted black. When the
aces are equally dark, and equally rough, there is no
arent difference. The thicker the pipe the more uniform
temperature; but on account of the expense they are
erally not more than ½ or ¾ of an inch thick.

Their arrangement.

. The expansion of cast iron pipes may be estimated at
of an inch for every 10 feet in length. Vertical pipes,
ng equally heated all round, continue straight; but hori-
tal pipes bend, because the upper side is heated most,
l this endangers the joints. Vertical pipes too may be
d as pillars for supporting the floors. In the arrange-
nt of the pipes two points require considerable attention.
st, conveniently to expel the air; and, secondly, to take
the water proceeding from the condensation of the steam.
en the steam enters the pipes, it acts as a piston, driving
air before it. This principle should be kept in view in
ng on the place of the opening for the escape of the air.

After the pipes are filled one or more openings will be necessary, to allow a small portion of steam constantly to escape, to keep up the heat of the pipes. If this be not done, air will accumulate, and occupy the place of the steam. It is best to make the condensed water run in the direction of the steam, which will drive the water before it in a horizontal pipe, or even in one with a considerable acclivity. Great care must be taken however, that no water lodges in the pipes: for. the water remaining in the pipes after they become cool keeps one part of them cold; the next time the steam is let into the pipes the regular expansion is prevented, some part of the pipe cracks, and a violent explosion takes place, racking the joints to a considerable distance in every direction. The common arrangement is to have a horizontal pipe going off separately from each vertical pipe. This requires an opening for letting out the air at the end of each horizontal pipe. A great improvement is first to carry the steam to the upper story in a vertical pipe, and close under the ceiling in a horizontal one nearly to the opposite end of the building; thence in a vertical pipe to the story beneath, and again horizontally under the cieling; and thus from story to story till it comes to the bottom; where the condensed water may be allowed to run off by an inverted siphon, which will allow the water to escape, while its pressure confines the steam. The air may be allowed to escape by a stopcock at the same place.

Mode of connecting them.

5. When the joints are formed by bolted flanches, these are liable to be broken from inequality of expansion, or to leak at the bolt-holes. Spigot and faucet joints do very well in some cases, but sometimes the faucets burst from the greater expansion of the spigots. If the ends of both pipes be included in thimbles, though these are equally liable to break, the expense is trifling compared with that of a whole pipe. For branch pipes the joinings should be made by saddles and hoops embracing the main pipe. Where there is much silk of unequal expansion, the joints should be secured by a soft stuffing of hemp, or cotton, and tallow; but in most cases they may be made with iron cement, composed of ion borings 40lbs, sal ammoniac 1lb, sulphur ½lb, well mixed together and beaten like putty.

The following is a Tabular View of the Effects of Steam in warming different Buildings.

Buildings.	Substancce of which the steam-pipes are made.	Cubic feet of space in building.	Cubic feet in boiler.	Space warmed by 1 cubic foot of boiler.	Cubic feet of space warmed by 1 superficial foot of steam-pipe.	Tempperatures, degrees of Fahrenheit in winter.
Messrs. H. Houldsworth and Co. Anderston Old Mill,	Cast Iron,	250000	———	2000	178	85°
Linwood, Ditto,	Cast Iron,	300000	120	2500	168	70°
Messrs. Kennedy and Watts, Johnston,	Cast Iron,	289000	160	1180	160	75°
Catrine,	Tin-plate not painted,	———	———	———	200	———
Mr. Thomas Houldsworth's Mill, Manchester,	Cast Iron,	———	———	———	195	———
Chapel of Port-Glasgow,	Cast Iron,	60600	10	6000	400	———
Part of Adelphi Cotton Works,	Cast Iron,	49140	———	———	182	65°
Tambouring Mill at Anderston,	Cast Iron,	———	———	———	240	60°
Messrs. William King and Sons, Johnston	Cast Iron,	244583	180	1303	200	70°

METEOROLOGICAL JOURNAL

For NOVEMBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Make
in the STRAND, LONDON.

OCT. Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest.	Lowest.		Night.	Day.
26	48	44	52	42	29·25	Rain	Rain
27	44	44	50	44	29·42	Fair	Ditto
28	44	45	50	43	29·39	Ditto	Ditto
29	44	44	49	42	29·66	Cloudy	Ditto
30	46	47	48	42	30·03	Ditto	Ditto
31	44	44	50	44	30·34	Fair	Cloudy
NOV.							
1	48	47	50	46	30·26	Ditto	Ditto
2	48	48	51	46	30·15	Cloudy	Ditto
3	47	47	50	42	30·04	Ditto	Ditto
4	44	42	47	38	30·21	Rain	Fair
5	40	37	41	32	29·90	Fair	Ditto
6	34	36	42	38	29·88	Ditto	Ditto
7	42	43	43	42	29·70	Cloudy	Ditto
8	46	48	50	45	29·65	Rain	Rain
9	47	50	52	50	29·56	Cloudy	Ditto
10	48	48	52	48	29·74	Ditto	Fair
11	46	45	48	38	29·84	Fair	Rain
12	42	41	44	40	30·10	Cloudy	Fair
13	40	38	41	32	30·11	Ditto	Ditto
14	34	37	38	34	30·11	Fair	Ditto
15	40	47	49	42	29·98	Cloudy*	Ditto
16	50	51	54	49	29·61	Ditto†	Ditto
17	50	51	51	46	29·27	Rain‡	Rain
18	46	43	48	33	28·80	Ditto§	Ditto
19	34	38	40	36	29 31	Cloudy	Ditto
20	40	46	50	45	29·73	Rain	Ditto
21	50	46	54	40	29·76	Fair	Fair
22	41	41	52	46	30·18	Ditto	Rain
23	47	50	52	46	30·12	Rain‖	Cloudy
24	46	45	50	44	30·19	Ditto	Ditto
25	48	51	52	46	30·00	Fair	Ditto

* 11 P. M. Rain and high wind.
† Ditto ditto.
‡ Ditto ditto.
§ Rain and fall of snow.
‖ Preceded by heavy mist.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXI.

ARTICLE I.

Essay on the Composition of Alcohol and of Sulphuric Ether.

By Theodore de Saussure.

(Concluded from p. 273.)

Decomposition of Sulphuric Ether.

Sect. VI. *Preparation of the Sulphuric Ether employed in my Experiments. Considerations on the Analysis of this Fluid.*

Preparation of the ether.

A HUNDRED parts of sulphuric acid by weight, mixed with a hundred parts of spirit of wine of the shops, the specific gravity of which was 0·842 at 16° of Reaumur [68° F.], afforded me by distillation through a worm 53 parts of ether not rectified, the specific gravity of which was 0·797.

Rectified.

This ethereous liquor, after mixing it with an alcoholic solution of potash, was distilled nearly to half at a temperature of 35° R. [111° F.]. The ether, freed from the sulphurous acid, oil, and a part of the alcohol which were united with it, was of the specific gravity of 740° at 16° R. [68° F.]. This is the rectified ether of the shops.

Washed.

The ether obtained by this operation was mixed with twice its weight of water *, to separate still more alcohol from it: and the ether, after it was decanted, was found to be reduced by this washing to the specific gravity of 0·726.

One third drawn off.

This last product subjected to distillation, and only a third of it drawn off, yielded an ether of the specific gravity of 0·717 at 16° R. [68° F.]. This was the ether I employed in my experiments.

More might have been obtained.

It is unnecessary to observe, that by repeatedly rectifying, and washing the residuums of the preceding rectifications, four or five times as much ether of the specific gravity of 0·717 might be obtained, as by confining ourselves to the first result mentioned above.

Results of burning in a lamp not accurate.

It has been seen, that the results of the slow combustion of alcohol in a lamp in a close vessel were deficient in precision. Those I obtained by the slow combustion of ether were still more so, and therefore I shall not detail them.

Its detonation gives more specific results than that of alcohol.

The analysis of ether made by detonating its elastic vapour appeared to me sufficiently accurate to determine the proportions of carbon, oxigen, and hidrogen. It is capable of giving more precise results than those obtained from alcohol by this process. The alcoholic vapour is so light, that its specific gravity is difficult to be ascertained. A very slight errour in determining it makes great differences in the results of the analysis. The gaseous vapour of ether is much heavier; all the results are more striking; and small errours here are less important.

Its decomposition in a red hot tube not quite so accurate.

The decomposition of ether by an incandescent porcelain tube afforded me less precise results than the preceding operation, and much less accurate than those obtained from alcohol by the same means, because the ether in this process yields thirty times as much oil, with respect to the composition of which I could only form conjectures. I will relate the particulars of this process however; as they will serve to confirm the analysis of ether by the rapid combustion of its vapour.

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* The efficacy of this method has been demonstrated by the experiments of Gay Lussac, given in Berthollet's Chemical Statics, vol. I.

SECT. VII. *Decomposition of Ether by an incandescent Porcelain Tube.*

Through a porcelain tube glazed within and heated red hot I passed 1103 grains of ether. I did not apply fire directly to the retort, from which the ether was distilled, for the vicinity of the furnace that heated the tube raised it to 27° R. [92·7° F.], and this temperature was sufficient to distil over the whole of the ether in the space of fourteen hours.

1103 grains of ether passed through a red hot porcelain tube,

The apparatus for this experiment was in all respects similar to that employed for the analysis of alcohol, and described in Sect. V. The porcelain tube was equal in size, and exposed to the same degree of heat in the same furnace.

as the alcohol had been,

The ether was entirely decomposed: at least no smell of ether was perceptible in the vessels, that received the products of the operation. It yielded me,

was entirely decomposed, yielding

1, In the middle of the porcelain tube 5¼ grains of charcoal, which separated in the form of a thin leaf or scroll. This charcoal, being incinerated in a platina crucible, left no ponderable quantity of ashes.

charcoal 5¼ grains,

2, In the glass worm and the upper half of the receiver about three grains of a very inflammable essential oil, crystallized in shining scales, transparent, and smelling of benzoin. Most of these crystals were contaminated by a brown empyreumatic oil, which they left behind after evaporating in the common temperature of the air.

volatile oil 3 grains,

3, In the end of the porcelain tube that projected beyond the furnace, in the worm, and in the receiver, where it was more abundant, 43 grains of an oil nearly black, partly fluid, and partly of the consistence of honey. This had a smell of benzoin mixed with an empyreuma; was soluble in alcohol, and insoluble in water; acrid, and, the lips being touched with it, it gave pain, and caused suppuration. When spread upon paper it dried, and, viewed through a microscope, exhibited small yellow crystals, which were not volatile like the preceding in the common temperature of the atmosphere.

another oil 43 grains,

4, A drop of water, weighing about three grains, found in the worm. It was colourless, smelt of benzoin, emitted

water 3 grains,

vapours at the approach of muriatic acid, produced no perceptible change in the infusion of litmus, or if it had any effect it was reddening it. There was no water in the receiver.

oxicarburetted hidrogen 948 grains,

5, Lastly, 3541 cubic inches of oxicarburetted hidrogen gas, at 27 inches 3 lines of the barometer, and 16° R. [68° F.]. In this there was no mixture of carbonic acid gas; but there came over with it into the receivers a thick yellow smoke, with a strong smell of benzoin and empyreuma. This vapour was partly lost in the water of the trough, on the top of which an insoluble pellicle was found floating, after it had stood a few days. When I detonated this inflammable gas immediately after it was formed, and while the vapour was suspended in it, it produced more carbonic acid gas, than when it was condensed. Every thing therefore indicates, that this smoke was the vapour of oil.

and a thick yellow smoke.

The gas weighed and analysed.

The gas was not weighed and analysed till twenty-four hours after its extrication, and the complete disappearance of the vapour. That which was formed in the first periods of the distillation was lighter, and contained less carbon, than what was produced toward the end, though the heat of the porcelain tube did not vary. On taking a mean between three portions of this gas weighed at the beginning, middle, and end of the process, I found, that 3541 cubic inches weighed 948 grains*.

Weight and composition of oxicarburetted hidrogen vary from several circumstances.

* At 28 inches of the barometer, and 10° of the thermometer [54 5° F.], 1000 cubic inches of oxicarburetted hidrogen gas weigh 285 grains. That obtained from a similar distillation by the Dutch chemists weighed under the same circumstances 326 grains. That which Mr. Cruickshanks produced weighed 297 grains. According to this author, 100 cubic inches of this gas consume 176 of oxigen gas in forming 108 of carbonic acid gas. Nothing is more variable than the weight and composition of this gas according to the degree of fire, the diameter of the incandescent tube, its inclination in the furnace, and the period of the experiment, at which the gas is collected. I conceive, if this gentleman had weighed and analysed it at every period of its developement, he would have found in it less carbon. I speak here of the quantity of carbonic acid gas produced by the combustion of the inflammable gas, and not of the absolute quantity of carbon he ascribes to it: this appears less than mine, because he reckons much less carbon in the carbonic acid.

Immediate products of the ether.

The immediate products of the decomposition of 1103 grains of ether then are,

	Grs.
Oxicarburetted hidrogen gas - - - -	948
Charcoal - - - - - -	5·25
Oil, in part volatile - - - -	46
Water - - - - - - -	3
	1002·25
Loss owing to the oily smoke carried off by the gas - - - - -	100·75
	1103.

The gas analysed.

On analysing the gas in all the receivers by Volta's eudiometer, and taking a mean of the several analyses, I found, that 100 parts by measure of this oxicarburetted hidrogen gas consumed for their combustion 145 parts of oxigen gas, forming with them water, and 88 parts of carbonic acid gas.

As to nitrogen gas, I found no more after the combustion, than I had added with the oxigen gas which produced it. Indeed most of my eudiometrical analyses indicated, that the nitrogen gas introduced previous to the combustion had undergone a small diminution in consequence of the detonation *. The drop of water found in the worm adapted to the incandescent tube exhibited signs of ammoniacal vapour on the approach of muriatic acid: but this test is often illusory; and besides, as it is impossible for me to affirm, that my ether contained no alcohol, the existence of nitrogen in ether must remain undecided.

Its composition

On applying the calculation adopted in § V for the analysis of the oxicarburetted hidrogen gas of alcohol to the

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Nitrogen condensed in some circumstances, not in others.

* This condensation of nitrogen, according to the experiments of Humboldt and Gay Lussac, does not take place in the detonation of pure hidrogen gas with atmospheric air. In our operations the circumstances differ, because the hidrogen is more condensed in the oxicarburetted hidrogen gas of ether, than it is in pure hidrogen gas. This diminution of nitrogen did not appear in the combustion of the inflammable gas of alcohol, either because this already contained nitrogen, or because the hidrogen is less condensed in the oxicarburetted hidrogen of alcohol than in the oxicarburetted hidrogen gas of ether, and even than in pure hidrogen gas.

results we have just obtained from the detonation of the oxicarburetted hidrogen gas of ether, we find, that 100 parts by weight of the latter contain,

Carbon - - -	56·12
Hidrogen - - -	17·43
Oxigen - - -	26·45
	100.

Ether contains more carbon and hidrogen, but less oxigen, than alcohol.

The analysis of this gas compared with that of alcohol is sufficient to give us some idea of the composition of ether, and show us, that this liquor contains in an equal weight more carbon and hidrogen, but less oxigen, than alcohol: for this oxicarburetted hidrogen gas alone constitutes more than three fourths of the weight of the ether I decomposed. The other fourth, which I pass by, is almost wholly oil, in part fixed, in part volatile, which must have some similarity of composition with the ether. But as oil, according to the analysis of Lavoisier, contains scarcely any thing but carbon and hidrogen, it follows, that, in referring the composition of ether to the proportions of the elements of the gas I have just analysed, we have too little hidrogen and carbon, and too much oxigen. This will be confirmed by the following process, which gives more precise results.

SECT. VIII. *Analysis of Ether by the Detonation of its Elastic Vapour.*

Vapour of ether detonated with oxigen gas.

For the preparation of the oxigen gas dilated by the vapour of ether, and the estimation of the weight of this vapour, I adopted the same processes as those I applied to the vapour of alcohol, § III. I think it useless therefore to repeat them; but I shall give one example of their results, the barometer being at 27 inches, and the thermometer at 18° [72·5° F.]. Of five experiments made in a similar way this appeared to me the most accurate, though their differences were slight.

Expansion of the vapour.

The elastic force of my ether, or the depression of the column of mercury by a drop of this fluid introduced into its vacuum, was 16 inches 9 lines. On applying to this result the formula of Dalton $\frac{p}{p-f}$, we find, that a volume of air equal to unity, into which the ether is introduced, will occupy, in consequence of the expansion of the ethereal vapour, a space equal to 2·6341. I obtained the same result by passing a drop of ether into a receiver full of air over mercury, and measuring this air both before and after the dilatation.

A thousand inches of atmospheric air dilated by the vapour of ether contained therefore 379·63 cubic inches of pure atmospheric air, which weighed 161·9 grains.

1000 cubic inches weigh 654·47 grains.

I found by a direct experiment, that 1000 cubic inches of atmospheric air dilated by the vapour of ether weighed 816·37 grains. Consequently 1000 cubic inches of pure ethereal vapour weigh in atmospheric air 816·37−161·9= 654·47 grains, according to the principle, that elastic vapour has the same weight in the air and in vacuo. (*See note at the end of this paper.*)

Oxigen gas dilated as much as possible with vapour of ether or of alcohol is not fired by electricity.

Oxigen gas dilated as much as it can be in the common temperature by the vapour of ether will not take fire by the electric spark. The reason is, the vapour of ether is too abundant, or, in other words, the oxigen gas too much rarefied. Alcoholized oxigen gas likewise does not take fire, but from an opposite cause, the alcoholic vapour being too much rarefied; for, on adding pure oxigen gas to the alcoholized oxigen, the vapour does not take fire, because it is still more rarified; but if pure oxigen gas be added to the etherised oxigen, the vapour inflames.

Vapour of ether fired with a large proportion of oxigen.

I mixed over mercury 100 parts by measure of etherised oxigen gas with 504 parts of oxigen gas, and detonated them by the electric spark. The explosion burst the eudiometers, which were not very thick. The 604 parts of aeriform fluid, which before the detonation contained 541·96 parts of oxigen gas, were reduced by their combustion to 344·31 parts, in which a second eudiometrical analysis showed there were 230·51 parts of carbonic acid gas, and 113·8 of oxigen gas. The residue of the first operation contained a dew, which appeared to be aqueous, and was void of smell.

Produce of 65·447 grains of vapour of ether.

One-hundred parts by measure of vapour of ether therefore consume 428·15 parts of oxigen gas*, leaving as a

residuum

Soot appears in some cases.

[*] If with etherised oxigen gas we mix a less quantity of oxigen

gas

residuum water, and 230·51 parts of carbonic acid gas. Hence we must conclude, that the oxigen gas has burned 395·28 parts of hidrogen gas contained in the ether.

Admitting the numbers I have given to represent cubic inches, and substituting for these the corresponding weights, the barometer being at 27 inches, and the thermometer at 18° [72·5 F.], we find, that 100 cubic inches of ethereal vapour weigh 65·447 grains, and contain,

Carbon.

1, The carbon of 230·51 cubic inches of carbonic acid gas, or 38·64 grains of carbon.

Hidrogen.

2, Hidrogen gas 395·28 cubic inches, weighing 12·62 grains.

Water.

3, A quantity of oxigen and hidrogen answering to 14·187 grains of water.

Its component parts.

On substituting for the water its elements, and proportioning all the results of the analysis to 100 parts of ether by weight, we find, that they contain

Carbon - - - -	59·04
Hidrogen - - - -	21·86
Oxigen - - - -	19·1
	100.

General results.

These results are reducible to the following: 10 grains of ether consume for their combustion 61 cubic inches of oxigen gas, at 28 inches of the barometer, and 10° of the thermometer [54·5 F.], forming water, and 32·85 cubic inches of carbonic acid gas.

The analysis I have just related was repeated four times, and a mean of the four indicates in 100 parts of ether

Carbon - - - -	58·2
Hidrogen - - - -	22·14
Oxigen - - - -	19·66
	100.

gas than will consume all the ethereal vapour, or merely sufficient for this purpose, a black powder, or soot, will be deposited on the sides of the eudiometer, and some free oxigen gas will remain in the aeriform residuum of the detonation. This soot does not appear, when the etherised oxigen gas is detonated with a quantity of oxigen gas much superior to what is requisite for burning the whole of the ethereal vapour.

SECT. IX. *Examination of the Water produced by the Combustion of Ether.*

Examination of the water

Hitherto I have taken it for granted, that the fluid residuum of the combustion of ether was water, but without any other proof, than a very superficial examination of the slight dew, which is formed in the eudiometer by the inflammation of the ethereous vapour. It remains for me to examine how far this supposition was well founded.

from several ounces of ether.

I burned several ounces of ether, in the apparatus invented by Meusnier to obtain the water produced in the combustion of alcohol. The water thus obtained from ether is without colour, smell, and taste, except some traces of empyreuma, which it loses by exposure to the air. It has the same specific gravity as distilled water, with which it mixes without becoming turbid. It is not precipitated either by nitrate of silver, lime water, or even acetate of barytes. When I evaporated one ounce of it to one fifth of its former weight, acetate of barytes produced a cloud in it incapable of being weighed.

Examined for sulphur.

To estimate the quantity of sulphur contained in sulphuric ether by another process, I dissolved one ounce of this liquid in fourteen ounces of water. A stream of oxigenized muriatic acid gas was passed through this solution for ten hours. The ether was in part decomposed, but the solution containing the products of this decomposition was rendered but slightly turbid by acetate of barytes, till it was reduced by evaporation to a quarter of an ounce. As the result is so trifling, it is impossible to conceive, that sulphuric ether can derive any of its essential properties from the presence of sulphur.

A little lead in the water from the worm.

The water obtained from ether by the apparatus of Meusnier was rendered turbid and of a deep brown colour by the hidrosulphuret of potash. This precipitate arose from the lead acquired from the worm of the apparatus.

Appearance of ammonia.

It emitted copious ammoniacal fumes at the approach of muriatic acid, and it appeared to me, that it changed the sirup of violets green in a very slight degree: but this change of colour certainly did not take place with the water obtained from the combustion of ether under the mouth of a glass jar. In the latter process the distillation is slower, because we lose a larger quantity of water raised in vapour; and that which is collected, having been longer exposed to the air, suffers more ammonia to fly off.

Further proof of its presence.

One ounce of the water obtained from ether in Meusnier's apparatus, and received in a bottle into which I had put a few drops of muriatic acid, in order to saturate the ammoniacal vapours during distillation, was evaporated to dryness in the temperature of the atmosphere. The residuum it left was dry and well crystallized muriate of ammonia, but mixed with a little muriate of lead. The muriate of ammonia, separated from the metallic salt by a fresh solution and crystallization, weighed one grain and three tenths. Here therefore its proportion was greater than in the water obtained from the combustion of alcohol, § IV.

Perhaps the ammonia not wholly from the ether.

Though it is possible, that ether may contain a little nitrogen, I doubt whether the ammonia, found in the aqueous product of the combustion, come wholly from the ether. Whatever care I have taken in my eudiometrical trials, I have not been able to satisfy myself, that the nitrogen gas is not condensed into ammonia in the combustion of the vapour of ether. My results on this point have not been uniform. The greater number have indicated this condensation, and I am inclined to admit it, because the manipulations and slight errours incident to Volta's eudiometrical process have a tendency to produce the opposite effect, in other words, to introduce nitrogen gas into the residuum of the detonation *.

The water evaporated left a little residuum.

I evaporated to dryness, in a very gentle heat, 288 grains of water obtained from ether burned under the mouth of a glass jar. It left as a residuum a transparent varnish, weighing at most an eighth of a grain, and attracting moisture from the air.

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* If we operate over mercury, there is always in this metal, and some interstices of the eudiometer, a little common air, which mixes with the residuum of the detonation, to fill the vacuum it produces. When the operation can be performed over water, the air contained in this fluid separates from it for the same reason, but it is in less quantity than from mercury.

It contained a little acetic acid.

To find whether the liquid I examined contained acetic acid, I added a few drops of potash to 288 grains of water obtained by the same process as the preceding. The solution was saturated with carbonic acid gas, then evaporated to dryness, and afterward washed with alcohol; when a white salt was dissolved, weighing 0·7 of a grain, and very speedily deliquescing. It had all the characters of acetate of potash.

All the foreign matters in it trifling.

Thus the experiments I have just related indicate in the water produced from the combustion of ether the presence of acetate of ammonia, a portion of sulphuric acid too small to be weighed, and a slight deliquescent varnish, the nature of which I could not ascertain. But the weight of all these substances taken together is so small with respect to the water holding them in solution, that it can make very little difference in the proportions of carbon, hidrogen, and oxigen, assigned to ether in my last analysis.

SECT. X. *Application of the preceding Analyses to the Inquiry concerning the Changes Alcohol undergoes in its Transformation into Ether.*

Comparison of alcohol with ether.

In considering the changes effected in the conversion of alcohol into ether, I shall regard only the proportions of oxigen, hidrogen, and carbon, neglecting the nitrogen; the existence of which in alcohol is certain, but questionable in ether, though it is demonstrated, that the water produced by the combustion of ether with the acid of atmospheric air contains a perceptible quantity of ammonia.

	100 parts of alcohol are composed, § V, of	100 parts of ether, § VIII, of
Carbon	43·5	59
Oxigen	38	19
Hidrogen	15	22
Nitrogen	3·5	
	100	100

Alcohol loses oxigen in becoming ether.

These results show, that in equal weights ether contains much more carbon and hidrogen, but much less oxigen, than alcohol does. Mr. Berthollet had already considered ether as a product, that must have more hidrogen and less oxigen than alcohol[*].

Residuum from the mixture of sulphuric acid and alcohol.

The residuum of the mixture of sulphuric acid with alcohol holds in suspension, after the ethereous fluid is separated, a bituminous or resinous matter [†] greatly loaded with carbon. It will be asked, no doubt, how it is possible, that ether should contain more carbon than alcohol, since the latter has let fall a portion of this element in its conversion into ether. But it must be remembered, that this residuum contains likewise oxigen and hidrogen, which are found either in the bituminous substance or in the state of water; and that, if this oxigen and hidrogen be taken from the alcohol in larger proportion than the carbon, the latter must predominate in the ether.

Two parts of alcohol yield one of ether.

To judge whether my analyses lead to this conclusion, I have examined what quantity of ether a determinate weight of alcohol would produce; and I found by approximation, that two parts of alcohol, if wholly decomposed, would give one of rectified ether. I obtained this result by the following operations.

80 p. alcohol, 20 water, and 100 sulphuric acid, produced 60 of impure ether.

A hundred parts of common spirit of wine of the specific gravity of 0·845, and containing 80 parts of perfect alcohol with 20 of water, produced, by mixture with an equal weight of sulphuric acid, 60 parts of ethereous fluid not rectified, by stopping the distillation at the moment when the sulphurous smell is decidedly perceived, and the oil begins to appear. I actually collected only 53 parts of the ethereous fluid, but I found, from the difference in the weight of the retort, that contained the spirit of wine and sulphuric acid, taken before and after the distillation, that 60 parts had been produced. It is well known, that a certain quantity of ether always flies off in vapour during this process, the weight of which could not be found in any other way. In the following operations I continued to estimate the weight of the product by that of the residuum.

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[*] Statique Chimique, vol. ii, p. 531 and following.

[†] *Ibid.* and Proust, Mémoires des Savans étrangers, Institut nat. vol. I.

The

These rectified to 25·25.

The 53 parts of ethereous fluid, which I suppose equal to 60, were mixed with liquid potash, and by the known processes of rectification afforded me 25·25 parts of ether.

Residuum yielded 10·3 more,

The residuum of this rectification, which must be equal to 34·75 parts, was separated from the potash by distillation. It was miscible with water in any proportion, and had nearly the specific gravity of common spirit of wine. I mixed it with an equal weight of sulphuric acid, and it produced 23·25 parts of ethereous fluid, which, having been mixed with potash and rectified, yielded 10·3 parts of ether.

and afterward 3·2.

In all 38·75 of pure ether.

The alcoholic residuum of this rectification was separated from the potash, and mixed for the third time with sulphuric acid. This afforded 3·2 parts of rectified ether. The 80 parts of perfect alcohol therefore produced in all these operations 25·25 + 10·3 + 3·2 = 38·75 parts of ether, or nearly half the weight of the alcohol employed. Ten parts of water did not entirely dissolve one of this ether. Its specific gravity was equal to 0·736 at 16° R. [68° F.]. I did not wash it with water, but it would certainly have been lighter, if I could have obtained the specific gravity of that which was volatilized. I have taken no account of a small quantity of spirit of wine, which, according to the observation of Proust, always remains mingled with the sulphuric acid after the first separation of the ether. I do not think therefore I shall be far from the truth, if I say, that 200 parts of perfect alcohol produce by their complete decomposition 100 parts of ether of a density equal to 0·717 at 16° R. [68° F.].

Elements left by the alcohol after the separation of the ether.

If we take the difference between 200 parts of alcohol and 100 of the ether produced from it, reducing the two liquids to their ultimate elements, we shall have a remainder equal to 100 parts, which, setting aside the sulphuric acid, express the elements left by the alcohol after the separation of the ether; and which include

Carbon - - -	28
Oxigen - - -	57
Hidrogen - -	8
Nitrogen	

This residuum then must contain a considerable quantity of carbon, though the ether is more loaded with it than the alcohol. It will no doubt be remarked, that this residuum contains oxigen and hidrogen nearly in the proportions that constitute water, or in that of 7 to 1. We must admit therefore, that 100 parts of ether are nearly equal to 200 parts of alcohol, minus 28 parts of carbon, and 65 of water, the formation of which has been occasioned by the sulphuric acid.

Not a perfect separation of the products.

The black substance precipitated from the alcohol is not, as has been said, pure carbon: nor does it appear, that the liquid formed with the elements of alcohol by the sulphuric acid is pure water. An imperfect separation of products takes place here, as in all decompositions effected between substances that have a very movable constitution, and little disposition to solidity.

The result approximations merely.

In this paper I have attained nothing farther than approximations, but in researches of so difficult a nature, these results are the only ones we can expect. They cannot acquire great precision, but by repeated analyses successively improved.

Note on the Vapour of Ether, § VII, p. 327.

Specific gravity of elastic vapours.

In a paper read to the Society of Natural History and Philosophy at Geneva, December, 1804, I gave the particulars of an experiment made for the purpose of ascertaining directly the specific gravity of the elastic vapour of ether in vacuo. The conclusions drawn by de Laplace from the observations of Watt, my father, and Gay-Lussac, show decidedly, that the elastic vapour of water is found in the same quantity in vacuo and in the air at the same temperature; but we cannot apply the same law to ether, except by analogy, or very indirect experiments. (See the experiments of Mr. Dalton on the evaporation of ether. Manchester Memoirs, vol. V, or our Journal, vol. VI, p. 266.)

Experiment to ascertain that of ether.

I procured a matras, the body of which contained 30 cubic inches, and the cylindrical neck of which was 32 inches long, and about three lines in diameter. On this neck I measured off a length of about two inches, and weighed the quantity of ether requisite to fill this length

The matras was filled with mercury, except a space equal to that of the small column, that had been measured, and this was filled with ether. I then closed for an instant the orifice of the matras, and inverted it in a basin of mercury, under which I opened it. Thus the matras became a kind of imperfect barometer, terminating above in a hollow ball void of air, but filled with the vapour of ether. The length of the column of ether, previously measured, was diminished above a third by the formation of the vapour I have mentioned. This diminution reduced to weight, and compared with the capacity of the body of the matras, gave me the bulk and weight of the vapour of ether in vacuo, and proved, that they were equal, at least as far as could be expected in an experiment on 30 cubic inches, to the bulk and weight of this vapour in atmospheric air, in nitrogen gas, and in hidrogen gas. The vapour of alcohol is too light, to afford sufficiently decided results by this process.

Necessary precautions.

In this experiment there are some precautions to be taken, in which however there is no difficulty, 1st, to expel from the surface of the mercury contained in the barometer a small quantity of liquid ether, which lodges between the mercury and the inside of the neck when the matras is inverted. This may be effected by surrounding it with a cloth warm enough to reduce this ether into elastic vapour.—2dly, it is necessary to estimate by a comparative experiment, made at the same time and in the same place, with a matras of equal size, the weight of the liquid ether that adheres in small quantity to the inside of the body filled with vapour. 3dly, in stopping the matras to invert it, the stopple must not touch the ether. I avoided this source of errour, by fixing in the neck of the matras, near its orifice, a tube closed at the bottom, and filled with the ether intended for this experiment.

2oz. of ether fill the space of a cubic foot.

I found thus, that a cubic foot void of air, or filled with air, could contain, at a temperature of 18° R. [72·5° F.], about two ounces of invisible ether in the state of gas. The extraordinary weight of this vapour instructs us how much ether is lost, by employing large vessels or globes passing one into another, for the purpose of condensers and receivers in distilling this fluid.

A know-

Specific gravity of vapours affords useful data.

A knowledge of the specific gravity of vapours may furnish considerable resources in chemical analyses. By the help of this datum, and detonating a few cubic inches of the vapour of ether with oxigen gas, I was able to determine with more precision the proportions of oxigen, hidrogen, and carbon in ether, than by burning two ounces of this liquor in a red hot tube. I obtained results nearly as accurate with the vapour of alcohol.

Vapour of ether employed to ascertain its affinities to pitch,

The vapour of ether may be employed with as little expense for determining the affinities of this fluid to different substances. I introduced over mercury 12 grains of pounded pitch into 20 measures of atmospheric air dilated by the vapour of ether, which consisted of 10 measures of air previous to its dilatation. The 20 measures occupied a column 6 inches high, and 6 lines in diameter; and were reduced to eleven measures by the presence of the dry pitch, which became semifluid in thus condensing almost the whole of the ethereous vapour.

suet,

I obtained a somewhat less condensation with 12 grains of suet, 20 measures being reduced only to 13. The suet was softened.

India rubber,

Twelve grains of caoutchouc, very minutely divided, reduced 20 measures to 15.

camphor,

Twelve grains of camphor reduced 20 measures to 16. The camphor was moistened.

wax,

Twelve grains of yellow wax reduced 20 measures to 16½.

lac,

The vapour had very little action on gum lac, 12 grains of this only reducing 20 measures to 19.

and tragacanth.

A similar quantity of gum tragacanth produced a condensation too small to be measured.

Specific gravity of vapours in the direct ratio of the volatility of the substances producing them.

The knowledge of the specific gravities of the vapour of water, of alcohol, and of ether, may give us an idea of the law, which the gravities of vapours follow in proportion to the volatility of the liquids from which they arise. Water at a given temperature is less evaporable than alcohol, and alcohol than ether. The elastic vapour of water is lighter than that of alcohol; and the vapour of alcohol is lighter than that of ether. The specific gravity of elastic vapour then, at equal temperatures, appears to be in the ratio of the volatility of the liquors that furnish them. The most volatile bodies are those, which, in similar circumstances, produce the heaviest elastic vapours.

Gasses mix uniformly without regard to specific gravity.

Observations made by several natural philosophers indicate, that gasses of different natures mix uniformly, and do not arrange themselves according to the natural order of their different specific gravities: but if this observation were unfounded, if the vapours that emanate from the terrestrial globe arranged themselves in the order of their gravities, those that belong to the least volatile bodies, as the earths and metals, would be those that would occupy the highest strata of our atmosphere, supposing its temperature uniform.

II.

Description of an improved Ship's Stove, by Mr. JOSEPH COLLIER, *No.* 11, *Crown Street, Soho**.

SIR,

I HEREWITH send you a model of an improved ship stove, which may also be employed in drying houses, &c., with more safety than those in present use.

I submit it to the inspection of the members of the Society, who, I make no doubt, will see its advantages, and am, Sir,

Your humble Servant,
JOSEPH COLLIER.

P.S. The expense of one twelve inches diameter will be about eight pounds.

Description of a ship's stove.

Fig. 1, *Plate* IX, represents the stove, with the front partly closed by the circular slide *A*, which is moved from the back by the brass handle *B*. *C* a movable plate attached to the slide *A*, now supported by the latch catching a pin,

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* Transactions of the Society of Arts, vol. xxv, p. 93. Fifteen guineas were voted to Mr. Collier for this invention.

by which means it acts as a blower to cause the fire to burn more briskly, but which slides down also to shut the fire up.

D another plate, now hanging on its latch, but which can be let down to shut up the ash pit or dish *I*, which can be drawn out when the side facings *FF* are pulled up. *G* a circular plate or cap, which slides so as to shut the chimney up close.

Fig. 2, The body of the stove with the slider *A* moved round to the back, and thus leaving the fire-place completely open.

Fig. 3, The ash-dish shown separate.

Fig. 4, One of the side facings taken out to show the figure *H*, which slides into a hole made in the corner of the stove to hold it.

III.

Account of a Floating Light calculated to save the Lives of Persons, who have the Misfortune to fall overboard in the Night from any Ship. Invented by Mr. WM. SHIPLEY, *Founder of the Society for the Encouragement of Arts, Manufactures, and Commerce*[*].

The machine described.

THIS floating light consists of a hollow vessel in the form of a boat, made of tinned iron plate, *a b*, *Fig.* 5, *Plate* IX, the joints of which are carefully soldered, so as to keep out the water. The boat is 27 inches long, 13 broad in the middle, and 12 deep, and is sufficient to support a man in the water. From the gunwale of the boat, on each side, projects a handle *c d*, soldered fast to it for the man to hold by.

e f is a metal ring connected with the boat by four upright pieces, within which is another smaller ring, turning on pivots, fastened to the ring *e f*, in the direction of the *boat's* length; the internal ring supports a small lantern, *g*, by an axis which passes through it, and is pivoted into the ring at each end, in the direction of the boat's breadth. By means of these rings the lantern will remain in a vertical position, independent of the boat's motion.

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[*] Transactions of the Society of Arts, vol. xxv, p. 94.

Its application.

On the first alarm of a man falling overboard in the night, the candle is to be lighted, and the machine lowered into the sea by the rope; if the man should be at a small distance from the ship, he may, by means of the rope, be taken on board immediately on his reaching the machine, if not, the rope may be secured on the iron reel, to prevent its unwinding, and cast off, and the light will direct the man where to find it, and holding fast by the two handles it will support him in the water.

Fig. 6, *h*, is a rope ladder, having a lantern attached to it, as well to direct the person in the water to the rope ladder, as to enable the persons who lower the ladder to let it down till the cross-bar *k* reaches the water: *l* is a hook to hang the floating light upon. *Fig.* 7, *m*, is the reel for the line, by which the floating-light is to be lowered.

The lamp always to be ready for lighting.

Directions for its use.

It is proposed, in order to make this float useful, that it be placed every night under the care of the officers on watch; that its lamp be frequently trimmed and supplied with fresh oil, and its wick moistened with oil of turpentine, in order that it may take fire with the least touch of a lamp or candle; and whenever the alarm is given of any of the sailors falling overboard in the night, the officer on watch may light the lamp in the lantern belonging to the float as expeditiously as possible, and let the float down by a small cord, wound upon an iron reel, into the water, till it has floated about one second of time, and the float is a little way out of the perpendicular of the small cord. He is then to secure the cord on the reel, to prevent its unwinding, and toss it overboard. The reel will sink down, and pull the line almost perpendicular, and thus it will not be liable to entangle the person when he swims to the float, who, when he has got hold of the handles of it, may move it very fast which way he will, only by striking his legs in the same manner as he does when he swims; and as the light of the lamp will be a certain guide for the person fallen overboard to find the float, so it will also direct them in the ship to find the man and float: And when the ship has tacked about,

and

and is come to the float, then the following method is proposed to take up the man and float into the ship: viz. A lantern, with a rope ladder, may be let down by a cord from the ship, till a cross-bar below the lantern touches the water, which may be seen by them in the ship by means of the light from the bottom of the lantern; and thus the man in the water may lay hold of the cross-bar, and fix his feet on one of the steps of the rope ladder, and he may then lay hold of the iron bar or handle of the float with one hand, and hang it on the hook of the rope, above the cross bar; which being done, the man and float may be both safely lifted into the ship.

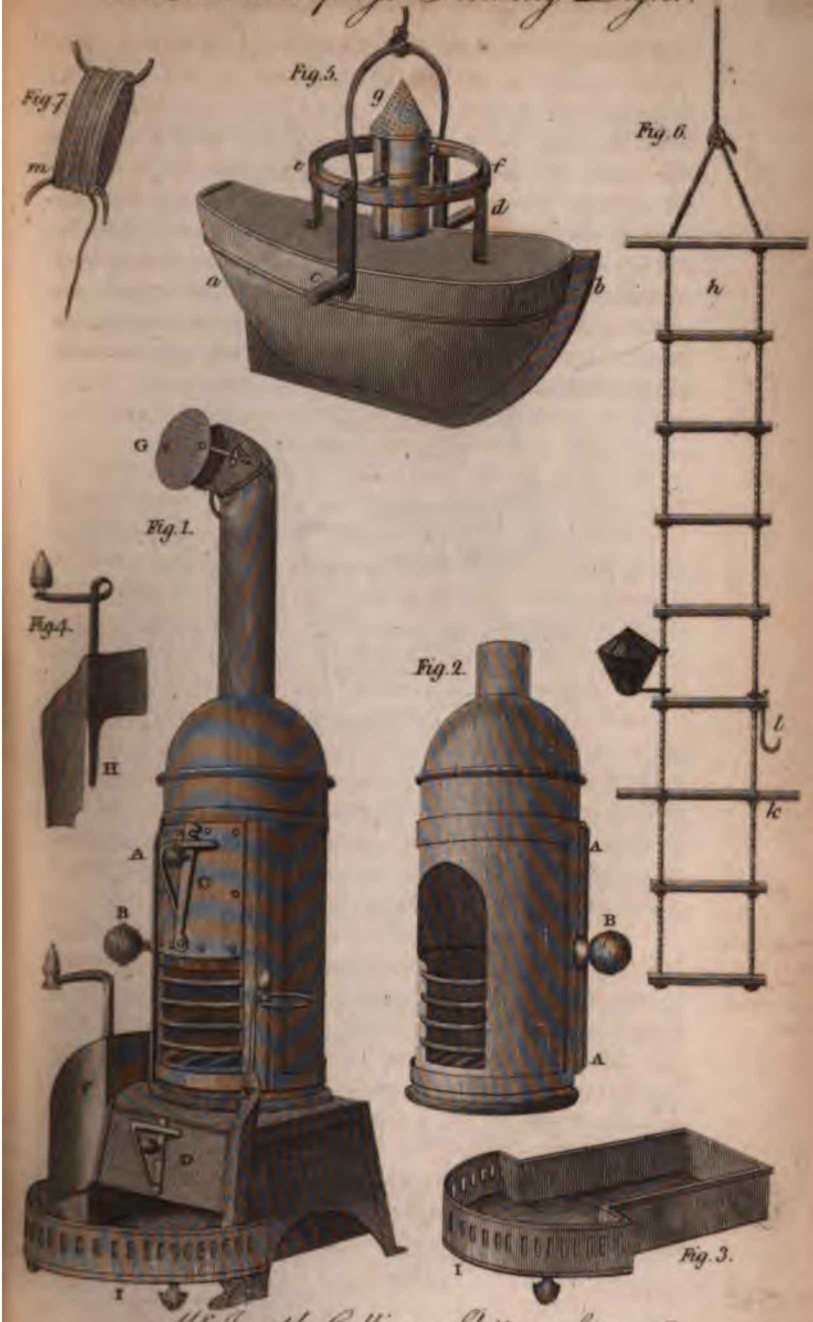
This ingenious and humane contrivance was presented to the Society by Mr. Shipley in 1776, and the silver medal, with a letter of thanks, was voted to him. The machine has been preserved in their repository, but as they consider it to be not sufficiently known, they have published the preceding account in their Transactions for last year. I remember observing, in the time of the American war, that several of our ships kept a small hull of a vessel lashed to the rails of their stern gallery, or their tafferel, ready to cut away the moment a man fell overboard. This hull had a single mast, with a red flag, that the waves might not conceal it from the sight of the man in the water; and was of course much preferable to the common resource, a hen-coop, or a grating. Such a flag might very easily be added to Mr. Shipley's floating light, for use in the day.

Ready contrivance to keep a person afloat that cannot swim.

While on this subject it may not be amiss to notice the contrivance, I believe of the late admiral Locker, by means of which a person who cannot swim may assist another in danger of drowning, and at least keep him afloat, till farther help can be obtained. If a man tie up his hat in a handkerchief, with the knots meeting in the centre of the opening of the crown, he may go into the water safely to assist another, holding the knots in one hand so as to keep the hat upright; for the air in the crown of the hat, while held in this position, will be sufficient to keep two from sinking.

IV. An

Mr. Wm. Shipley's Floating Light.



Mr. Joseph Collier's Ship's Stove.

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IV.

An Essay on the Sugar of Grapes; By PROFESSOR PROUST.

Concluded from page 316.

If the importation of sugar were stopped, that of the grape might be substituted.

AFTER observing, that sugar is become an indispensable article of consumption, Professor Proust expatiates on the necessity of finding a substitute for that of the West Indies, should their intercourse with Spain, France, and other continental countries be cut off; and for this purpose he recommends the sugar from grapes. This he confesses is not precisely the same with that of the cane, but may very well supply its place. Without being refined it will answer every purpose, in which colour is no object, as for sweetening coffee, chocolate, or dishes made of milk, in pharmaceutical preparations, &c.

Its qualities.

When refined, says Professor Proust, it is perfectly white, but will not acquire the solidity of that of the cane, on account of its granular and porous crystallization; so that it cannot be made into loaf sugar, unless the art of the sugar-baker furnish him with resources, which I have no room to expect from the trials I have made.

Its sweetness is evidently inferior to that of the sugar from the cane, so that it must be used in larger quantity; and it is not so readily soluble. It dissolves entirely in spirit of wine; but it separates from it much sooner than that of the cane, and always in tuberculous, granular crystals, in which no determinate arrangement of parts can be perceived.

Contents of the unripe juice.

Presuming, that a comparison of the juice of green grapes with that of the perfectly ripe fruit will not be uninteresting, I shall first give a sketch of the results I obtained by analysing it. In it are found, 1, tartar; 2, sulphate of potash; 3, sulphate of lime; 4, citric acid in abundance; 5, malic acid a very little; 6, extractive matter; and, 7, water.

Would furnish acid of lemons.

The citric acid is the chief base of this juice. It contains neither gum nor saccharine matter: and in those years when the dearness of lemons does not allow us to extract their acid in Scheele's mode, the juice of unripe grape may be employed for the purpose with more advantage than has been supposed.

This converted into sugar and gum,

But the warmth of the weather promotes the maturity of this juice; the citric acid gradually disappears, so that scarcely any traces of it can be discovered in the ripe grape; and the products that occupy its place are the two species of sugar mixed with a little gum. The elaboration of the juice therefore consists in transforming this acid into gummy and saccharine products, in proportion as the fruit approaches maturity.

by parting with oxigen?

The elements of the citric acid do not differ from those of sugar and gum, as has been discovered; but, since analysis has found likewise, that it contains oxigen, or the acidifying principle, in more abundance than the nutritious products that assume its place, does this acid, during the ripening, merely lose a part of its oxigen, so as to approach nearer their nature? or does it raise itself to the same point by acquiring a larger proportion of carbon? This admirable metamorphosis passes before our eyes every year, yet nature has covered it with a veil impenetrable to them. To return to the fruit of the ripe grape.

or acquiring carbon?

Contents of the ripe juice.

This juice, as it flows from the fruit in the press, contains substances of two kinds, some simply mixed, others in solution. The parts mixed are, first, the fibrous and calcareous pulp, which composes the organization of the berry; and, secondly, a portion of the fecula, which we call glutinous, on account of its resemblance to the animalized substance of cheese termed gluten.

These two substances, if diluted, may be separated by the filtration of the juice, though imperfectly, on account of its viscosity, and their tenaciousness, which choak up the filter. But they may be separated much better by heating the juice to ebullition, because they coagulate, and rise to the surface. When scummed, and strained through flannel, the substances remaining dissolved in the clarified juice are,

1, A portion of fecula; 2, crystallizable sugar; 3, sugar not crystallizable; 4, gum; 5, extractive matter, either white, or tinged red, according to the species of grape.

When

Its rob.

When the juice of grapes is boiled down as far as can be done without danger of altering its qualities, it affords a rob, the quantity of which is proportional to the saccharine quality of the grape, and varies from 18 to 32½ per cent. It is difficult however to avoid some degree of empyreuma, particularly if the juice be acidulous. This alteration diminishes the quality of fermenting in the rob redissolved in water, though without annihilating it, as Beccher had concluded from his experiments. The liquid sugar of the cane too, as Duthrone informs us, is much sooner altered by boiling than the solid sugar.

Crystallizes.

The rob boiled down to a certain point crystallizes in a short time. It congeals into a spongy mass, more or less moistened with a sirup, that has a tendency to drain off. Its crystals, when drained, are a mixture of tartar and crystallizable sugar. It was this product, extracted from the muscat grape of Fuencarral, which, after having undergone a few purifications, led me, instructed as I was by Duthrone's excellent work on sugar, to treat the juice of grapes like that of the sugar-cane,

Mode of extracting the sugar.

As the juice contains acids, that hinder the extraction of the sugar, the first step is to free it from these. After the must has been scummed, and while it is nearly boiling, a lixivium of wood ashes is to be added by little and little, as long as any effervescence takes place. The acids may be known to be saturated by tasting the liquor, which will then have only a saccharine taste. It is then to be boiled down to about half, and left to cool in vats, or even in the copper boilers, for there is no danger of verdigrease as in preparing the rob. While it thus stands, the tartar and citric acid, if there were any, being converted into salts of difficult solution, subside with the excess of the ashes, and the sulphate of lime that was in the juice of the grape. The malic acid, converted into malate of lime, remains in the liquor in consequence of its great solubility.

Not to be boiled too much.

Clarification.

The must prepared in this way indicates 25° or 26° on the areometer. If it were boiled down beyond this, the subsequent clarification would not be so easy, on account of its thickness. It is then to be beaten up with whites of eggs or bullocks blood, heated, scummed, filtered, and boiled down to the consistence of a sirup, which may be more or less thick, according to the use for which it is intended. This rob, digested of its principal acids, answers as we see to the first product of the cane, saturated and boiled down to the point at which it takes the name of muscovado.

Must boiled down has a slight acrimony, and in a little time becomes solid.

When the must has been thus prepared, it affords us a coloured sirup, though extracted from white grapes. Its taste is sweet and pleasant; but if as much as a spoonful be swallowed, it affects the throat with that slight impression of acrimony, which is experienced from yellow honey. It condenses in eight, fifteen, or twenty days, mose or less, according to the degree to which it is boiled down, into a yellow, granular mass, of sufficient consistency to be pressed into pots, without flowing out if they be set upside down. The sirup that has not been most boiled is the first to become solid. The sugar of grapes appears to require a certain quantity of water for its crystallization, as it is not found in sirup too much boiled. Hence this is longer before it becomes solid, but then it acquires a consistency more convenient for carriage. Lastly, in this state the muscovado of grapes has the consistency, colour, and appearance of that of the sugar-cane. A vessel that contains but sixteen pounds of water will hold twenty five of this sugar, so that its specific gravity is to that of water rather more than as three to two.

For this it should not be too much boiled.

Sugar of the grape compared with that of the cane.

If muscovado of the grape be compared with that of the sugar-cane, we find, that the latter adds to a slight bitterness a peculiar aroma, the character of which is very striking in rum: while that of the grape has no sensible aroma, being a sugar with a flavour of roasted fruit. This taste, as well as its colour, is owing to the concentrated extractive matter; which has the common property of its genus, that of becoming darker coloured, both by simple exposure to the air, from which it attracts some principle, and by being heated. It is this that gives the muscovado of the grape its orange colour; an effect similar to which is produced in the sugar of the cane, the juice of which is nearly colourless. If the muscovado of the grape be diluted with a quantity of water equal to what it has lost, we obtain

btain a regenerated must far darker coloured than the resh: but it is to be observed, that the latter, if a coniderable surface of it be exposed to the air, soon acquires similar tint. These effects are peculiar to the extractive rinciple, the saccharine and gummy being insusceptible of it. Hence it follows, that the change it experiences from these causes united must extend to muscovado, and communicate to it, as to all roasted fruits, more taste and colour. The following is the proportion of the products discovered in this muscovado by analysis.

Its component parts.

	lb.	oz.
Crystallizable sugar - -	75	
Fluid sugar - - -	24	7
Gum - - - - -		5
Malate of lime - -		4
	100	

A little extract.

The quantity of extractive matter could not be estimated, but it must be very little, since the melasses, notwithstanding its colour, is perfectly transparent.

Proportions of solid and fluid sugars.

To discover the proportion of the two sugars I employed the following means. I set to drain heaps of muscovado, evaporated to such a point as experience had taught me was most favourable for the separation of the sirup, or fluid sugar. The latter, collected and kept some time secured against evaporation, has still let fall pulverulent sugar, and in such a quantity, that, from many experiments of this kind, I am persuaded the crystallizable sugar is more than seven eighths of the muscovado. Notwithstanding this, I have not thought proper to set it down above at more than three fourths; and this I mention, that more confidence than it deserves may not be placed in a process that could not possibly be accurate.

The gum separated by alcohol, and then the malate.

But it is not thus with its other component parts, the gum and the malate. If to a hundred parts of muscovado reduced to the state of a thin sirup alcohol be gradually added, the gum is first deposited. The fluid being decanted off, and more alcohol added, the malate will fall down. As I have frequently repeated this experiment with quantities of sixteen hundred grains, I have reason to believe, that the proportion of these is given pretty accurately in the table.

If

Separation of the solid sugar from the alcohol imperfect.

If the alcoholic solutions of the muscovado be kept covered with a paper only, the solid sugar will separate from it by crystallization, but never so completely as to be able to calculate the quantity, because the fluid sugar retains a good part. The same thing, as has been seen, takes place with honey thus treated.

Gum of the grape.

The gum of the grape is without taste or colour, and does not differ from what I have found in apples, mulberries, medlars, apricots, plums, &c. It is one of the nutritious products of vegetables, resembling gum arabic.

Malate of lime no injury to the sugar.

The malate of lime, we see, is but in small quantity. If the mixture of an earthy salt in a substance intended for food should be thought an inconvenience by those, who have no idea of the composition of vegetables, I would observe to them, that this salt exists in a great number of fruits, particularly the melon and love-apple; that the sulphate of lime is found in much larger quantity in most of our pulse, in wine, in the waters we most prize at Madrid, in several fruits, in the apple, medlar, quince, potato, &c. without having the least effect on our health.

Sugar of the grape sweetens less than the common.

As a condiment the muscovado of the grape does not sweeten as much as common sugar, on account of the water of crystallization it contains, and the inferior sweetness of its crystallizable sugar. To sweeten a pint of water as much as custom requires, two ounces of the sugar from the cane are sufficient; but two ounces and half of that of the grape are necessary; and with these proportions both the solutions mark the same degree on the areometer.

Contains neither free acid, alkali, nor tannin.

The solution of this muscovado changes neither the infusion of litmus nor solution of isinglass. Muriate of tin precipitates from it the colouring principle; as it does that of the juices of the carrot, melon, grape, sugar-cane, and all fruits.

Its uses.

It is very well adapted to milk, coffee, and chocolate; which it sweetens agreeably, without giving them any particular flavour, that can be disliked, as yellow honey does; and the slight acrimony mentioned in the beginning disappears, because it is only the effect of the extractive matter too much boiled down.

The muscovadoes I have examined were extracted from white grape, called *alvilla*, and the black, called the ragon grape. The first afforded twenty-six per cent, second thirty. The latter is not perceptibly higher co- ured than the other, as the skin of the grape alone is co- ured, if care be taken not to mix with the must the juice tracted by pressing. It will perhaps excite surprise, that must, after being freed from its acids, affords a quantity muscovado equal in weight to the rob: but the reason of s is, that the tartar, the only acid that precipitates with lime, and a few particles of calcareous citrate and sul- ate, are found in it but in very small quantity. Of this may judge by the following result, though we may pre- ne there is a little more in the common grape than in the iscadine. A pound of the latter duly treated with spirit wine does not afford more than 48 grains of tartar.

Black grapes afforded more than white, and scarcely darker coloured.

Very little tartar in the grape.

It is not the tartarous acid, but the malic, that gives ipes their sharpness: and this too is but in small quantity, ce a pound of the juice of the muscadine grape does not ord above 40 or 45 grains of malate of lime. Now if reckon, that this salt contains one third of its weight earth, it will follow, that a pound of the fruit does not ntain much above 30 grains of acid.

Their acid the malic.

Hence we may conclude, that the juice of the grape freed m its tartar, an effect that may be obtained by simply iling it down to one third, is already a muscovado little ferent from that of the cane, which equally contains ma- acid, if no lime have been employed in its preparation.

Grape juice simply boiled down to one third almost wholly sugar.

the sugar of the grape approaches so near that of the ne in its qualities, we may understand why the rob of the iscadine, dried and poured on a marble, affords a trans- rent lozenge, without colour, pleasant to the taste, and pearing like barley sugar: but it has the defect of soon owing moist, as the malic acid and liquid sugar occasion to deliquesce in a short time.

Transparent lozenge from the rob.

It is remarkable, that the common people have already pproached very near the art of making grape sugar, in the reparation of their rob; but the last step, that remained or them to take, required a kind of reflection, for which their education is seldom adapted. At Arganda, near Madrid,

The art of sugar making nearly approached by the common people.

drid, and in other places, to prepare their rob they begin by boiling separately with a certain quantity of lime the juice of grapes, and that of other fruits they intend to mix with it. Thus, taught by necessity to free them from the acids, that would injure the sweetness of the rob, they employ a process truly chemical, to which theory, so long preceded by practice, cannot refuse its sanction.

The grape muscovado an excellent remedy for scurvy.

The muscovado of the grape will some day no doubt be used for other purposes beside food, when it is known, that in it are united the two vegetable products acknowledged to be best adapted for effectually remedying those diseases that are occasioned by the corruption of the blood, or that impoverishment of the humours called scurvy. The employment of the two kinds of sugar with a particular view to ascertain their effects, particularly freed from all the Galenical farrago that might weaken their powers, may furnish the physician with means of cure better adapted to his views, than those imaginary antiscorbutics, that still continue to usurp the place of efficacious remedies, than those salads of scurvy grass, brook-lime, and water-cresses, the heating acrimony of which could not fail to kindle consumptive fires, if the sick to whom they are prescribed were not protected from these by the dissipation of the qualities of the drugs by our infusions, clarifications, and sirups. Let us hear what Tourlet says, speaking of the scurvy:

General remedies for scurvy

"Fresh vegetables, pure air, aliments that contain most of the mucoso-saccharine principle, always infallibly cure the scurvy. The mucoso-saccharine principle contained in most fresh vegetables, in honey, in sugar, and in various fermentable substances, is of all things best calculated for assimilation, and consequently for the regeneration of the fibrine of the blood.

Animal food not always most nutritious.

"Animalized substances are not always the best fitted for nutrition: on the contrary, those are more so, that require for their animalization a sort of fermentation, which elaborates them, and renders them more capable of being assimilated with the substance of the individual, who uses them as food. Children, for instance, thrive much better on mucous and fermentable substances, than on such as are more animalized. Experience, against which there is no arguing, has incontrovertibly proved, that the use of meat is always pernicious to the scorbutic."

Refining the grape muscovado.

The refining of grape sugar must differ but little, if at all, from that of the muscovado of the cane. Both being composed of two sugars, that require to be separated, nothing is required but to boil down the prepared must to a proper degree of consistency, which every refiner by trade will discover. The muscovado of the grape, brought to this point, will condense within a few days into a cellular granulous mass, the intervals of which will be filled with fluid, the common effect of that attraction, which induces the particles of the two sugars to unite with those of their own kind, and separate into two products. These masses being drained, the result is sugar in its first stage of refinement and sirup. The latter, exhausted by fresh crystallizations retains the malate of lime, gum, and extractive principle. These four substances equally form the melasses of the sugar-cane; but that of the grape has not the same unpleasant flavour.

Cannot be made loaf sugar.

The sugar of the grape however does not crystallize like that of the cane; its grain is pulverulent; and as the masses it yields have little consistency, it appears to me doubtful, whether it can ever be brought to such a degree of hardness as that of the cane: at least it would require management, with which I am unacquainted.

Dissolved in water it ferments spontaneously:

If the sugar of the grape in this point of view afford us prospect of an important article of trade, the product of s fermentation promises us no less advantage. Nature has ven this muscovado such a tendency to fermentation, that requires nearly the addition of as much water as it had st, to produce this effect: and in cold countries, where e warmth necessary to this purpose is deficient, if a little ied wine-lees be added to this regenerated must, its fer- ntation will be still more active, and then it will proceed briskly as in temperate climes.

and one part produces 4 of a strong wine.

One measure of this muscovado dissolved in three of ter forms a liquor of equal density with the juice of the ragon grape, which indicates 17° on the areometer. produces four measures of a wine of the colour of of Malaga, and in which a slight flavour of baked

fruit is perceptible. It is as strong as the best w
la Mancha. As it is extremely intoxicating, certain
ther the beer nor the mead of Russia can be put in c
tition with it for strength or goodness. The muscov
the grape therefore may furnish the north with a base
ted to the manufacture of all sorts of wine.

The skins of the black grape colour and improve it.

If the skins of black grapes be added to this, it fe
with equal briskness, and acquires not only their c
but a portion of their astringent principle, which
derate quantity improves the taste of all wines, and
quality of keeping.

Valuable therefore in northern countries.

This muscovado imported from the south into the
solves a problem of great importance to cold cou
This is, that with the sugar of the grape wine may in
be made in Siberia as readily as in the kingdom of
sia. And if this production were considered only as
terial for making brandy, what advantage would it
in the ease and safety of conveyance! Would not be
be much improved, if its fermentation were promote
portion of this muscovado[*]?

Barley contains little soluble matter.

[*] The meal of barley contains but ten or eleven per cent
ducts soluble in cold water. These consist in equal parts
and mucoso-saccharine matter, rendered acrid by a little ext
and a few flocks of glutine that separate while boiling.

Its farina.

The farinaceous part consists in two or three and thirty
starch, and seven or eight and fifty of a granular insipid sub
which is separable from the starch by washing either in cold
ing water.

Distilled.

By distillation it yields all the products of starch, with so
dications of ammonia. Nitric acid employed without heat
cates from it a very little nitrogen.

Malt contains more soluble matter,

Barley that has been perfectly malted does not yield as
ten or eleven per cent of soluble products, but thirty pe
though of the same nature.

and less starch.

The farinaceous part consists of seven or eight and fifty
starch, and twelve or thirteen of the granular substance.
changes produced in the grain by germination therefore fall
The same substance is found in the flour of Indian corn, an
stitutes near half its bulk.

Not much sugar in it.

As the gummy part has no share in the fermentation, and
found in the beer, malted grain contains only about fifte

Glauber said the rob of grapes would make wine.

he celebrated Glauber asserted in his *Prosperitates Ger-*
iæ, that, if the rob of grapes were sent to countries,
hich nature has denied the vine, they might make their
wines, by adding to this quintessence of wine, as he
ed it, the water of which it had been deprived. And
aid this might be done in all places, and at all seasons.

Beccher denied this.

his idea was certainly ingenious, but he should have
firmed it by experience. He did not; and was openly
tradicted by Beccher in terms not very civil, who as-
ed, that he had tried the experiment in vain for a whole
r.

It will ferment however, but not readily if too much boiled.

n defense of Glauber it may be said, that the sugar in
rob, being more or less affected by the reaction of the
ar and other acids, remains so long inactive, as to lead
belief of its fermentable property being extinct. Not-
standing this however, it will ferment, and the period
be accelerated easily by the addition of wine lees. I
e even now some wine from such a fermentation, which
ery strong, and the boiling down has given it a flavour,
is far from unpleasant. But in some parts of Germany
grape has the double inconvenience of being loaded with
ar, and poor in saccharine matter, since it requires six
of must to make one of rob; probably therefore it
ld not be so much disposed to ferment as in hot coun-
, in Spain particularly, where the poorest juice of the
e commonly yields a fourth part of sugar and very lit-
cid.

t may not be improper to introduce here the remarks I
had an opportunity of making during the course of a
summers on the fermentation of clarified must.

Clarified must

Vhen the juice of the grape has been clarified by heat
filtration alone, it always continues a little foul, be-

---

of saccharine matter. If now we compare barley malt with
uscovado of the grape with respect to their fermentable parts,
hall find, that one hundred weight of the latter nearly equal
a hundred weight of the former. Hence we may judge of the
ntage, that would accrue from employing a portion of this
ovado in making beer.

ater heated to 50° does not dissolve starch: this is the reason
the water in brewing is seldom allowed to exceed this point.

cause it retains in solution a portion of the fecula that been mentioned, and the nature of which has been pletely ascertained by Fabbroni and Thenard. This f is retained there apparently by the intervention of a since we do not find it in the juice, that has been satu by the carbonate, and clarified with whites of eggs which way alone it is obtained perfectly clear.

ferments though freed from fecula.

Fabbroni and Thenard have considered this fecula ferment indispensable to the change of the saccharine ter: but when the juice of the grape has been care freed from it, the fermentation takes place as briskly must not clarified, and we find it pass through all its s in the same period, without depositing any thing but trite of lime.

The cause of the fermentation is in the liquid sugar.

The true cause of fermentation in juices, whether c fied or not, does not reside in this fecula therefore, b the fluid sugar, the only principle of fruits that is truly mentable of itself, and capable of imparting this move to solid sugar. Deyeux appears to me to be the first observed this difference, and it must be confessed, tha the phenomena of fermentation tend to confirm his opin Let us take a rapid view of them.

1st stage of fermentation.

The first effect of fermentation on a juice that has clarified but not saturated is the absorption of the first tions of carbonic acid, that begins to be evolved. product occasions the honied sweetness to be succeeded a brisk taste, which, without being spirituous, render must far more pleasant than it was before; and it is in state, that children like it so much.

2d stage.

The second is the increase of the bulk of the liquor a temperature exceeding that of the atmosphere, th diminished by all the heat the carbonic acid gas carries and the opacity of whey not well clarified.

3d stage.

At the third period the spirit of wine begins to ap and then the presence of this frees the must from its fe and a great part of its tartar. The gum, extractive ma and malic acid subsist amid the fermentation, without ing the least part in it, since we find them in the same portions after it is over.

Filtering checks the fermentation, but it again revives.

The fecula.

If the wine be filtered when at its greatest degree of opacity, its fermentation is perceptibly checked; but it afterward revives, and pursues its course without depositing any thing but particles of fecula and pure tartar. This fecula, or second lees of wine, is always loaded with tartar: but when it has been copiously washed, we find in it all the characters on which Thenard has insisted, and particularly those appearances, that have led Berthollet to compare it with starch. It is perfectly insoluble; grows sour, ferments, and acquires the bad smell of the gluten of wheat; in a word it becomes cheese. When it is dry it is a little transparent, horny, and affords all the products of animalized matters. Potash dissolves it, and separates it from the parts that are purely fibrous. In fine, it is the same thing as the unclarified must rejects in the first moments of fermentation; and if it do not separate from it at the same period, it is because its solubility retains it in the liquor, till the alcohol comes to precipitate it. Other circumstances confirm the fact, that this fecula is no more necessary to the transformation of the two sugars into alcohol, than the former, or than the gum, extractive matter, tartar, &c. If we take must saturated and clarified with whites of eggs, fermentation commences in it the next day. It pursues its course without depositing any fecula, but tartrite of lime alone; and without yielding any thing but carbonic acid.

The liquor grows lighter. Its residuum.

In the space of a month the liquor falls from 17° on the areometer to 1° or 2°. If we analyse the residuum after distillation, we shall find again the gum, malic acid, extractive matter, vinegar, some remains of sugar, and nothing more.

Fecula has nothing to do with fermentation.

The muscovado brought to 17° by a sufficient quantity of water ferments completely, changes into wine, and deposits but a few particles of matter. Where then is the influence of the fecula, the tartar, the acids, and the extracts? But the best clarified must will no doubt retain a portion of fecula; and it may be said, that this excites fermentation in the sugar. If this be the case, I would answer, the fermentation should be weaker in proportion to the loss of this principle occasioned by the clarification of the must; but we do not find, that this is at all behind that which retains the whole of its fecula. Hence let us conclude, that the fecula is one of those products, which are not necessary to fermentation, and that one of the first effects of this change is to free the juices from it, as it frees them from the tartar and sulphate: that, if fermentation required some of the other products of vegetation, to enable it to produce its due effect, it is much more natural to suppose, that those which their solubility renders injurious to the sugar would take a part, than an insoluble substance, which we always find again subsequent as well as previous to it, and of which not the least traces are to be found in wine or its products.

The fecula altered by fermentation.

The fresh fecula of the grape mixed with a solution of sugar at 17° is incapable of fermentation, as Berthollet and Thenard have already observed. I have also ascertained this fact. But if with such a solution of sugar we mix the same fecula after wine has fermented on it, or after it has become lees, it will excite a very brisk fermentation in it in a few hours.

Fecula of the 2d stage.

The white and muddy fecula deposited in the second stage of fermentation does not dissolve in the fermenting liquors; it undergoes no decomposition in them; it changes neither its bulk nor appearance; and there is no trace of it discoverable in the wine. It appears to take no part in the phenomena of fermentation, yet it impresses on crystallizable sugar the fermentative motion. In this case we see clearly, that it acts as matter impregnated with a principle which it transmits. What then is this principle? All that remains for us is to examine, whether we can divest fecula or lees of this impregnation, this leaven, which fits them for exciting fermentation; to enable us afterward to determine, whether the lees themselves really possess this property, or whether they act only by virtue of this principle, in which case they are merely a vehicle. This is a point on which Seguin appears to be occupied.

Does it contain a principle of fermentation.

The gluten not affected by fermentation.

In several spirituous fermentations, in which I have employed yeast, or meal, the gluten has always risen to the top, and adhered in shreds to the mouths of the vessels; and I could easily perceive, that it had neither altered its nature, nor been affected by the changes of the fermenting medium.

I have

Melasses of the grape ferments of itself.

I have said, that the liquid sugar was fermentable *per se*. Melasses from the muscadine grape, separated from its crystallizable sugar, has not lost the property of fermenting. Alone and simply dissolved in water, notwithstanding having been tortured by a number of evaporations, and treatments with chalk and spirit of wine, and its extractive principle having acquired an extremely disagreeable acrimony, it has notwithstanding afforded a strong wine.

The crystallized sugar not tried alone.

I have not yet tried to ferment the crystallizable sugar of the grape, to ascertain whether it be fermentable *per se*: this is a step I mean to take, as soon as I have a sufficient quantity; but I suspect beforehand, that it is not any more than the sugar of the cane.

Tartar not necessary to fermentation.

The tartar is a product of vegetable elaboration, like all those that accompany it in the juice of the grape, but it is not a necessary ingredient of fermentation. If nature had intended it to concur in its phenomena, she would not have given it that slight solubility, which occasions its separation in the beginning, when the sugar would have the most need of its influence. Glauber was well convinced of this; and accordingly he recommends the separation of the tartar from the rob, after diluting it in warm water; "for thus," he says, "it will be freed from its acidity, and the wine will be rendered sweeter." It is surprising that Glauber, who had considered the subject so well, did not think of saturating the must.

Bullion asserted, that it was essential.

This controverted by facts.

The experiments, on which Bullion is desirous of establishing the necessity of tartar, have led him to consequences much better calculated to increase the vague ideas respecting fermentation, of which we have already too many, than to elucidate its theory. If the tartar contributed to the alterations of the sugar, we must admit, that the part it acts is purely mechanical, since we find it entire after the formation of the wine. We cannot avoid surprise at the assertion, that must would not ferment without tartar, from one who had daily before his eyes the fermentation of apples, pears, the sugar-cane, services, oranges, gooseberries, cherries, and all kinds of fruit, the juices of which are destitute of tartar; as well as that of honey, of sugar assisted by yeast, and of malted grain. His analyses are not more conclusive. What too must be the quality of the grapes, the juice of which afforded him but four drachms of sugar to the pint? and this sugar, how could he characterize it as of the same species with that of the cane? Farther, on what grounds can he talk of fermentation, and its produce in spirit, from trials in which we find sugar employed in the proportion of one pound to fifty quarts of water? The liquorice water, that children sell by the shell-full, is not poorer stuff.

Tartar added to the juice of sour grapes will not make wine,

or increase the produce of spirit.

If it were true, that fermentation caused the tartar to concur in the production of wine, and even that it could consume fresh quantities for this purpose, as Bullion asserts, we ought never to meet with it in our casks; and the juices most abundant in tartar, those of the years in which the grape does not ripen fully, would afford wines most abundant in brandy. If we could believe, that doubling the quantity of tartar would occasion the produce of spirit to be half as much more, what better use could we make of this salt, than adding it to the must in the proportion of half a drachm to a quart, the dose that he asserts occasioned his obtaining half as much more brandy?

Price of the grape muscovado.

With regard to the price at which the muscovado of the grape can be afforded, thirty pounds, under the most unfavourable circumstances, and making full allowance for every thing, cost at Madrid 45 reals [20*s.* 1¾*d.*]; but had every thing been bought at the best hand, and the laboratory been a place fitted up for the purpose, the cost would certainly not have exceeded 30 reals [13*s.* 5¼*d*]: and in what part of the kingdom of Spain is coarse sugar or even honey to be bought for a real [5*d* ⅜] a pound? Add to this the tuns of grapes annually wasted in the country. At Toro, this year, I am told, that the beggars, after being glutted with grapes that they could not consume, left above 170000 arrobes [about 2125 tuns], or about 50000 arrobes [625 tuns] of muscovado. And at Aranda de Duero 2000 cantars [500 galls.] of wine, that could neither be sold nor consumed, were thrown into the kennels; and 150000 were left in the vineyards.

Lime dissolves in spirit.

A fact that should not be omitted is the solution of lime in spirit, which I believe has not been observed. I distilled twenty five pints of red wine of la Mancha, adding a handful of quicklime, to obtain at once a product free from the vinegar, which is always found in the first distillation: but the brandy came over so strongly imbued with the smell and taste of the lime, that I was surprised. This spirit in fact contained lime, as was demonstrated by all the tests; and its solution was so far from the effect of some unobserved circumstances, that, when I redistilled it with a gentle heat, it rose again with all its disagreeableness. Even now, after the lapse of three years, the spirit is not altered; it precipitates the metallic solutions, and oxalic acid, and restores the blue colour of litmus reddened by an acid. This solution then is a new point of similitude between the earths and alkalis.

Purification of the grape juice.

I have only to add, that recent trials have taught me nothing more is necessary, to saturate and clarify the juice of the grape, but to throw some powdered chalk into it, agitate the mixture, and let it stand till the next day. The fecula and earth will unite; the juice is then to be strained, boiled, and scummed; and whites of eggs are unnecessary.

The least boiled crystallized the soonest.

Desirous of knowing what degree of boiling down was most favourable for the crystallization, I made five experiments in the following order. Having clarified and saturated some juice, I boiled down one portion of it so as to leave but thirty-two hundredth parts of extract; another to thirty-four hundredths; a third to thirty-five; a fourth to thirty-six; and a fifth to forty. Of these the last crystallized first, next that of thirty-six, and then that of thirty-five. Those of thirty-two and thirty-four have not crystallized yet. Hence it is evident, that those sirups, which are least boiled, are the first to yield their sugar.

Soup.

Meat-soup contains fifty per cent of a savory extract, analogous to the product I have obtained from the fermentation of cheese and gluten. This extract is the condiment, the perfume, the quintessence of the soup: is that of bones comparable to it?

V.

Account of a simple Improvement in the common Still. In a Letter from Mr. J. Acton.

To Mr. NICHOLSON,

SIR,

Improvement in stills.

I SEND you an outline of an improvement I have added to my common still and worm tub, which I have found of such great utility, that I cannot resist the desire I have of communicating it. The still holds about nine gallons, and is used for distilling common water, essential oils, and water impregnated with them. The tub holds about 36 gallons, and not being near any water, I was accustomed to have a great deal of trouble in changing that in the tub when it became hot, which it did very soon after commencing the operation. It was this trouble, that put me to the necessity of contriving the additional condenser, which, though very simple, I have found to answer every purpose I could wish; and I can now distil any length of time without the water in the tub being scarcely raised a degree in temperature, or requiring to be changed, as the heat accumulates in the additional condenser, and when elevated to about 140° or 150°, passses off by evaporation.

This condenser consists of a trough, *a*, *Plate* X, *Fig.* 1, three feet long, twelve inches deep, and fifteen inches wide; with a pewter pipe, *b*, passing through the middle of it, of about two inches diameter at the largest end, and gradually tapering to about three quarters of an inch at the smaller end. It is most likely, that so simple and so useful a contrivance must have been thought of before, but never having seen or heard of any, I have taken the liberty of troubling you with it, requesting you wlll exercise your own judgment as to the propriety of inserting it in your Journal, as I have so firm a reliance upon the justice of it I cannot be otherwise than pleased with your decision.

I remain, Dear Sir,

Your obliged and faithful Servant,

J. ACTON.

Ipswich,
August 19, 1808.

VI. *Description*

VI.

Description of a new Variety of Carbonate of Lime. By R. J. HAUY *.

71 varieties of carbonate of lime known.

IN my Treatise on Mineralogy I described forty-seven determinate varieties of form in carbonate of lime. About two years after I published in the Annals of the Museum of Natural History a memoir containing a description of thirteen more varieties of the same substance, making together sixty; and since that time I have observed eleven, equally marked with novelty, so that at present the number of forms presented by this species amounts to seventy one.

Above 8 millions possible,

This number is trifling to what theory demonstrates to be possible, which exceeds eight millions, even supposing we confine ourselves to the four simplest laws of decrement. But I am far from considering the formula, that has led me to this result, as exhibiting the sum of past and future discoveries; and we need not fear being perplexed at some future period by our abundance, if we consider, that among the circumstances capable of determining the production of this immense quantity of crystalline forms there are a great number, that do not exist in nature. The formula to which I allude merely shows us how fertile the laws of the structure of crystals are in themselves; and teaches us, that Science has in her hands certain means of determining with precision all the new forms, that may present themselves to mineralogists in the course of their researches, however varied they may be, and however little analogy they may bear in appearance to those that are already known.

but circumstances requisite to the production of all do not exist.

Only a small part of things possible actually exist.

The very steps that Science takes in her progress, in proportion as she enriches herself by discovery, indicate, that what does exist is confined to very narrow limits compared with what is capable of existing. The new varieties of carbonate of lime, that have been found within these few years, are almost all of them but different combinations of laws already observed, and the greatest of these combinations does not include above six quantities.

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* Journal des Mines, vol. XVIII, p. 299.

Only two laws to add to those before known.

Trihexaedral carbonate of lime.

Prismatic hyaline quartz.

In the applications I have made of the theory to these varieties I have found but two laws to add to the twenty-one I had mentioned in my treatise as those, of which I had then recognised the existence. The first determines in part the structure of a variety, which I have called trihexaedral carbonate of lime, because its figure is that of a six-sided prism, terminated by right pyramids of the same number of faces. Three of these faces are parallel to those of the primitive rhomboid, and the other three, which have the same inclination, result from a decrement by two rows in height on the inferior angles *e*, *Fig.* 2, *Plate* X, of the same rhomboid, so that if this law attained its limits, the secondary form resulting from it would be similar to the nucleus. This structure is likewise that of the prismatic hyaline quartz, which I have described in my Treatise on Mineralogy, vol. ii, p. 411; but in the quartz crystals the inclination of the terminal faces to the adjacent sides is 141° 40′, while in the carbonate of lime it is only 135°; which arises from the difference that exists between the primitive forms themselves. I am indebted to Mr. Héricart-Thuri, mine-engineer, for the knowledge of this interesting variety, a specimen of which he has presented to me.

New variety of carbonate of lime.

The second law relates to the variety, that forms the subject of this article. The crystals, that have enabled me to determine it, were sent me from Clermont-Ferrant by Mr. Augustus Mabru, whose useful researches in the department of Puy-de-Dôme, as well as those of his worthy friend Mr. de Laizer, afford new proofs of the mineralogical treasures contained in that country. I avail myself of this opportunity, to render them both a public testimony of my gratitude for their eagerness to impart to me the fruits of their discoveries, particularly with respect to the species sulphate of barytes, of which they have sent me a considerable number of varieties hitherto unknown; and this pleasure has been greatly enhanced, as the symbols representing the laws of their structure, given in the letters accompanying them, generally announce observers equally attentive and enlightened.

Its figure,

Fig. 3 represents the variety in question. Mr. Mabru had very justly remarked, that it exhibited a rhomboid with equal axes, each of the six lower edges of which was replaced by a bevel with two facets, s, s''. Hence it follows, that, if we suppose these facets prolonged till they meet, so as to conceal the faces g, g, of the equiaxal rhomboid, the crystal would be a dodecaedron with scalene triangles, analogous to that of the metastatic variety, commonly called dog-tooth spar; and if we were farther to imagine planes passing through the edges x, x, &c., these planes would intercept a rhomboid similar to the equiaxal, and which with respect to the dodecaedron would have the same position, as the primitive rhomboid has with respect to the metastatic dodecaedron; so that the equiaxal rhomboid may be considered as a hypothetical nucleus with respect to the dodecaedron before us. We have already an instance of a hypothetical nucleus of the same kind in the paradoxal carbonate of lime discovered by the learned Mr. Tonnellier, keeper of the mineralogical collections of the Council of Mines. But in the latter variety the hypothetical nucleus is the inverse rhomboid; and it is remarkable, that the forms hitherto exhibited by these sorts of hypotheses are engrafted, as it were, on the two secondary rhomboids, the most simple among those that belong to carbonate of lime.

and structure.

It was easy to see at once, that the facets s, s'', must depend on a law intermediate to the angles $E E$ of the nucleus, *Fig.* 2, which is likewise the case with the paradoxal variety. Now in this there are two lines of particles subtracted from the edges $D D$, and only one from the edges $B B$, which is the most simple of combinations of this kind: and if we add the condition, that the hypothetical nucleus is the inverse rhomboid, it necessarily follows, that the intermediate decrement takes place with a single row. In the variety discovered by Mr. Mabru the two terms of combination are greater by unity than in the preceding, that is, there are three lines of particles subtracted from the edges $D D$, and two from the edges $B B$; and combining these data with the condition, that the hypothetical nucleus is the equiaxal rhomboid, we find, that the intermediate decrement is at the same time mixed, and takes place by five rows in breadth and six in height. Any other law would give for the nucleus a rhomboid different from the equiaxal. For instance, if we suppose the intermediate decrement to be made by a single row, the hypothetical nucleus would become a rhomboid extremely flattened, in which the great angle of the rhombus would be 119° instead of 114° 18′, and the greatest angle of incidence between the faces 160° 26′ instead of 134° 23′: and besides, on this supposition the values of the angles of the dodecaedron would differ very sensibly from those that give the law of $\frac{5}{6}$, which agree with observation.

Measures of its angles.

The following are the measures of the saliant angles. Between *g* and *g*, 134° 23′ 38″; *s* and *s*″, 118° 29′ 4″; *s* and *g*, or *s*″ and *g*′, 143° 32′ 39″; *s* and *s*, 115° 1′ 44″; *s* and *s*′, 142° 24′ 6″.

Numerical carbonate of lime.

I give this variety the name of numerical carbonate of lime, on account of the properties of the numbers that express its form; the sum of the exponent of *B*, which is 2, and of the exponent of *D*, which is 3, being equal to the numerator of the exponent of *E*, which is 5, and their product being equal to its denominator, 6.

I have likewise investigated the law that would govern the dodecaedron, if the hypothetical nucleus were substituted for the true; and I have found, that in this case the symbol of the dodecaedron would be $\overset{\frac{5}{3}}{D}$, a quantity the exponent of which is double that of *E* in the preceding case.

Possibility of substituting a secondary form to the primitive one.

In my Treatise on Mineralogy, vol. ii, p. 15 and following, I have developed the theory respecting the possibility of thus substituting a secondary form to the primitive one, so as to derive any other secondary form from it by the laws of decrement. This view gives an infinite scope, if I may be allowed the expression, to the results of that branch of geometry, which arises from the study of the laws, to which the wisdom and power of the supreme Being has subjected the formation of the regular bodies, that people the subterranean world; and our admiration increases, when we see this immensity of results end in one common term, the invariable form of the particle shown by the dissection of crystals.

This variety easily dissected.

I ought to say here in particular, that none of the varieties of carbonate of lime exhibit the rhomboid of 101° 30′ by the help of mechanical division with more facility, and more neatly, than that which has been described,

Mr.

Numerical Carbonate of Lime.

Fig. 3

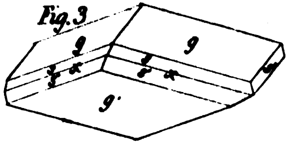
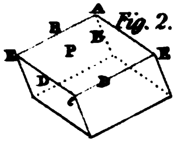
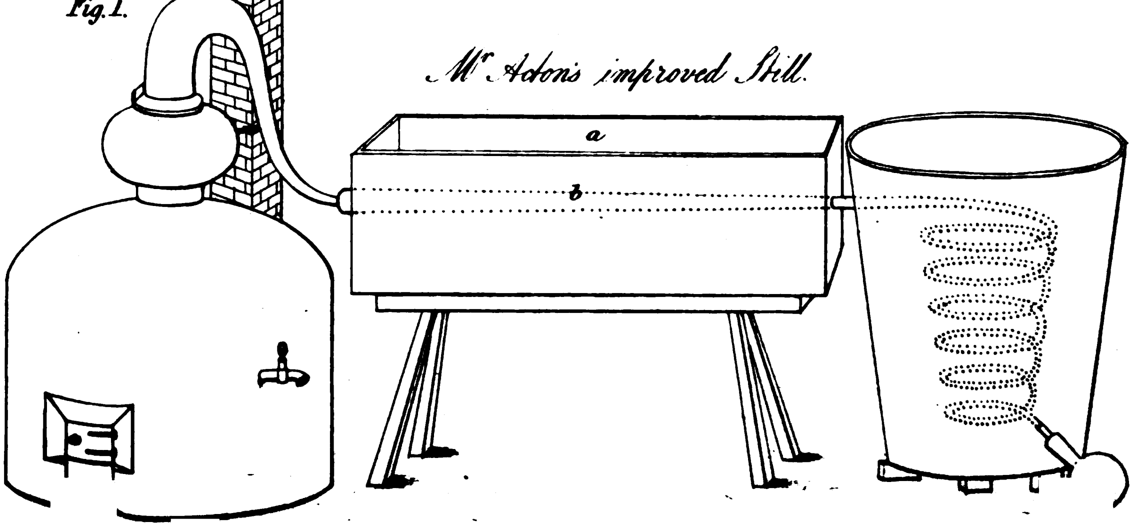


Fig. 2.



Mr Acton's improved Still.

Fig. 1.



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Where found.

Mr. Mabru found this variety at the foot of Puy-Saint-Romain, below the plaster quarries of St. Maurice, about ten miles south-east of Clermont, in the department of Puy-de-Dôme. Its gangue is a compact carbonate of lime of a gray colour, mixed with a little argile and oxide of iron. The largest crystals I have observed, are about 18 mill., or 8 lines [7 lines Eng.] broad. In the same place is found equiaxal carbonate of lime without any modification.

Size.

VII.

Second Letter on the Subject of the New Metals.

By Mr. A. Combes.

To Mr. NICHOLSON.

SIR,

The question respects historical documents.

As you have candidly admitted a letter into your Journal, in which your own statements (as it seems) are censured, you will not, I trust, refuse a place to my reply to your observations *. It may be considered as presumption in an obscure individual, to enter into the lists with a veteran in science; and this would be the case, were the question of any other nature than merely concerning historical documents; upon these topics the mere man of leisure may have an advantage over the man of business and genius; and to refer to authorities requires no great intellectual exertion.

Fourcroy's testimony equivocal.

I still maintain, in opposition to your opinion, for which on all other occasions I have the highest respect, that the true *alkalis* "were never long ago suspected to be metallic oxides."

You mention the testimony of Fourcroy, but the passage to which you refer is undoubtedly equivocal.

Mr. Fourcroy says, Systeme des Con. Chem. II, pag. 196, "l'opinion sur la pretendue nature metallique de la Barite ainsi que celle des autres bases salifiables surtout terreuses ne sera qu'une hypothese." Here "des autres" ought,

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* See Journal, p. 231—4.

merely to encourage a waste of time and a tendency to dreaming. Mere barren hypothesis, that neither arise from facts, nor lead to experiments, are *weeds* in the field of science which will always grow sufficiently without manure.

You, as an experimental poilosopher and a lover of truth, ought to endeavour to check their growth; and should your Journal be made a hotbed for their cultivation, it must inevitably loose its ancient universally acknowledged utility and importance.

I am, Sir,
Your obedient humble Servant,
A. COMBES.

Chelsea,
November 17, 1808.

VIII.

Electro-Chemical Researches, on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.* *

1. *Introduction.*

IN the Philosophical Transactions for 1807, Part I †, and 1808, Part I, I have detailed the general methods of decomposition by electricity, and stated various new facts obtained in consequence of the application of them.

Decomposition of the fixed alkalis led to hopes of similar results.

The results of the experiments on potash and soda, as I stated in my last communication to the Society, afforded me the strongest hopes of being able to effect the decomposition both of the alkaline and common earths; and the phenomena obtained in the first imperfect trials made upon these bodies countenanced the ideas, that had obtained from the earliest

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* Philosophical Transactions for 1808, Part II, p. 333.

† See Journal, vol. XVIII, p. 321, and XIX, p. 37.

‡ Ibid. Vol. XX, p. 290, 321.

French admit Mr. Davy's claim to the discovery.

o the foundations of a discovery, if not to the discovery tself, than our neighbours on the continent, yet on this ccasion they have been anticipated at home: for in a report f the Polytechnic School, published in the last Number of he Phil. Magazine, it is said by the editors of the journal of the Polytechnic School, "that Mr. Gay Lussac, and Mr. Thenard, had repeated Mr. Davy's experiments, and obtained the two new metals, of which the existence had not been suspected previous to Mr. Davy's experiments."

Mr. Davy's discovery.

You say, it is no derogation to Mr. Davy's merits, that he has explored the processes of nature by simplicity of investigation, and clear deductions grounded upon a knowledge of the antecedent analogies. On the last part of this proposition I cannot agree with you. It would in my opinion have been a derogation to his merit, had he been guided by any analogies so loose as those, which might have led him to look for metals in the fixed alkalis. He was on the contrary enlightened by new principles of research, arising from the knowledge of the properties of chemical decomposition by Voltaic electricity, which your useful labours partly led the way to, and which his discoveries have made almost universal.

The negative pole attracts only inflammable matter.

I attended his course of lectures of 1807, and in referring to my notes I find, that he stated it as a fact, that all bodies of known composition attracted by the negative pole in the Voltaic circuit consisted principally of inflammable matter, and were naturally positive; and that it was probable therefore, that all bodies of unknown composition attracted by this pole, and which were naturally positive, might also contain inflammable matter.

This he looked for in alkalis.

In his lectures in 1801, he stated, that, in looking for *nflammable matter* after those ideas in the fixed alkalis, e had *discovered* it, and that he had likewise found what e had not expected, that it was metallic in its nature.

In this instance sagacious conjecture and sound analogy ere followed up by experimental research, and ended in great discovery.

Guesses in science.

Guesses, except from experimental inquirers, ought arcely to be tolerated in science; and to attach import-ce to them, and to dignify them with approbation, is merely

From the infusibility of the earths.

The earths like the fixed alkalis are nonconductors of electricity; but the fixed alkalis become conducting by fusion: the infusible nature of the earths, however, rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxigen, made it unavailing, to act upon them in solution in water; and the only methods, that proved successful, were those of operating upon them by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity in metallic alloys, so as to obtain evidences of their nature and properties.

A more powerful apparatus wanted.

I delayed for some time laying an account of many of the principal results which I obtained before the Society, in the hopes of being able to render them more distinct and satisfactory; but finding that for this end a more powerful battery, and more perfect apparatus than I have a prospect of seeing very soon constructed, will be required, I have ventured to bring forwards the investigation in its present imperfect state; and I shall prefer the imputation of having published unfinished labours, to that of having concealed any new facts from the scientific world, which may tend to assist the progress of chemical knowledge.

2. *Methods employed for decomposing the alkaline Earths.*

The alkaline earths moistened, and electrified under naphtha. Inflammable gas evolved, and metallic points appeared.

Barytes, strontites, and lime, slightly moistened, were electrified by iron wires under naphtha, by the same methods, and with the same powers as those employed for the decomposition[*] of the fixed alkalis. In these cases, gas was copiously evolved, which was inflammable; and the earths, where in contact with the negative metallic wires, became dark coloured, and exhibited small points having a metallic lustre, which, when exposed to air, gradually became white; they became white likewise when plunged under water, and when examined in this experiment by a magnifier, a greenish powder seemed to separate from them, and small globules of gas were disengaged.

In these cases there was great reason to believe, that the earths had been decomposed; and that their bases had combined with the iron, so as to form alloys decomposible by the oxigen of air or water; but the indistinctness of the effect, and the complicated circumstances required for it, were such as to compel me to form other plans of operation.

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[*] See page 4, or Journal, vol. xx, p. 201.

Potassium tried to attract the oxigen from the earths.

The strong attraction of potassium for oxigen induced me to try whether this body might not detach the oxigen from the earths, in the same manner as charcoal decomposes the common metallic oxides.

Ineffectual.

I heated potassium in contact with dry pure lime, barytes, strontites, and magnesia, in tubes of plate glass; but as I was obliged to use very small quantities, and as I could not raise the heat to ignition without fusing the glass, I obtained in this way no good results. The potassium appeared to act upon the earths and on the glass, and dark brown substances were obtained, which evolved gas from water; but no distinct metallic globules could be procured: from these circumstances, and other like circumstances, it seemed probable, that though potassium may partially deoxigenate the earths, yet its affinity for oxigen, at least at the temperature which I employed, is not sufficient to effect their decomposition.

Potash and the earths tried to form an alloy.

I made mixtures of dry potash in excess and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit in the same manner as that I employed for obtaining the metals of the alkalis. My hopes were, that the potassium and the metals of the earths might be deoxigenated at the same time, and enter into combination in alloy.

The results rather more distinct.

In this way of operating, the results were more distinct than in the last: metallic substances appeared, less fusible than potassium, which burnt the instant after they had formed, and which by burning produced a mixture of potash and the earth employed; I endeavoured to form them under naphtha, but without much success. To produce the result at all required a charge by the action of nitric acid, which the state of the batteries did not permit me often to employ*; and the metal was generated only in

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The Voltaic battery weakened by use.

* The power of this combination, though it consisted of one hundred plates of copper and zinc of six inches, and one hundred and

very minute films, which could not be detached by fusion, and which were instantly destroyed by exposure to air.

As potash mixed with metallic oxides was rapidly decomposed,

I had found in my researches upon potassium, that when a mixture of potash and the oxide of mercury, tin, or lead, was electrified in the Voltaic circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium was obtained; the attraction between the common metals and the potassium apparently accelerating the separation of the oxigen.

the earths were mixed with similar oxides.

The idea that a similar kind of action might assist the decomposition of the alkaline earths induced me, to electrify mixtures of these bodies and the oxide of tin, of iron, of lead, of silver, and of mercury; and these operations were far more satisfactory than any of the others.

Barytes 2 p. oxide of silver 1 p.

A mixture of two thirds of barytes and one third of oxide of silver very slightly moistened was electrified by iron wires; an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point. When the iron wire to which this substance adhered was plunged into water containing a little alum in solution, gas was disengaged, which proved to be hidrogen; and white clouds, which were found to be sulphate of barytes, descended from the point of the wire.

Barytes and red oxide of mercury.

A mixture of barytes and red oxide of mercury, in the

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and fifty of four inches, at this time was not more than equal to that of a newly constructed apparatus of one hundred and fifty of four inches. It had been made for the demonstrations in the Theatre of the Royal Institution in 1803; and since that time had been constantly employed in the annual courses of lectures, and had served, in different parts, for the numerous experiments on the decomposition of bodies by electricity, detailed in the Bakerian Lectures for 1806 and 1807, and a number of the plates were destroyed by corrosion. I mention these circumstances, because many chemists have been deterred from pursuing experiments on the decomposition of the alkalis and the earths, under the idea that a very powerful combination was required for the effect. This, however, is far from being the case; all the experiments detailed in the text may be repeated by means of a Voltaic battery, containing from one hundred to one hundred and fifty plates of four or six inches.

same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance, that produced barytes by exposure to air, with the absorption of oxigen; and which occasioned the evolution of hidrogen from water, leaving pure mercury, and producing a solution of barytes.

Lime, strontia, and magnesia with the same oxide.

Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air or water, with like phenomena; but the quantities of metallic substances obtained were exceedingly minute; they appeared as mere superficial formations surrounding the point of the wire, nor did they increase after the first few minutes of electrization, even when the process was carried on for some hours.

A new battery employed.

These experiments were made previous to April, 1808, at which time the batteries were so much injured by constant use, as no longer to form an efficient combination. The inquiry was suspended for a short time: but in May I was enabled to resume it, by employing a new and much more powerful combination, constructed in the Laboratory of the Royal Institution, and consisting of five hundred pairs of double plates of six inches square.

When I attempted to obtain amalgams with this apparatus, the transmitting wires being of platina, of about $\frac{1}{40}$ of an inch in diameter; the heat generated was so great as to burn both the mercury and basis of the amalgam at the moment of its formation; and when, by extending the surfaces of the conductors, this power of ignition was modified, yet still the amalgam was only produced in thin films, and I could not obtain globules sufficiently large to submit to distillation. When the transmitting wires were of iron of the same thickness, the iron acquired the temperature of ignition, and combined with the bases of the earths in preference to the mercury, and metallic alloys of a dark gray colour were obtained, which acted on water with the evolution of hidrogen, and were converted into oxide of iron, and alkaline earths.

While I was engaged in these experiments, in the beginning of June, I received a letter from Professor Ber-

Pontin and Berzelius negatively electrified mercury in contact with barytes and lime.

zelius of Stockholm, in which he informed me, that, in conjunction with Dr. Pontin, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them, and that in this way he had obtained amalgams of the metals of these earths.

This repeated with success.

I immediately repated these operations with perfect success; a globule of mercury, electrified by the power of the battery of 500, weakly charged, was made to act upon a surface of slightly moistened barytes, fixed upon a plate of platina. The mercury gradually became less fluid, and after a few minutes was found covered with a white film of barytes; and when the amalgam was thrown into water, hidrogen was disengaged, the mercury remained free, and a solution of barytes was formed.

The result with lime, as these gentlemen had stated, was precisely analogous.

The same tried with strontia and magnesia.

That the same happy methods must succeed with strontites and magnesia, it was not easy to doubt, and I quickly tried the experiment.

From strontites I obtained a very rapid result; but from magnesia, in the first trials, no amalgam could be procured. By continuing the process however for a longer time, and keeping the earth continually moist, at last a combination of the basis with mercury was obtained, which slowly produced magnesia by absorption of oxigen from air, or by the action of water.

The amalgams might be preserved some time under naphtha.

All these amalgams I found might be preserved for a considerable period under naphtha. In a length of time, however, they became covered with a white crust under this fluid. When exposed to air, a very few minutes only were required for the oxigenation of the bases of the earths. In water the amalgam of barytes was most rapidly decomposed: that of strontites and that of lime next in order: but the amalgam from magnesia, as might be expected from the weak affinity of the earth for water, very slowly changed; when a little sulphuric acid was added to the water, however, the evolution of hidrogen, and the production and solution of magnesia were exceedingly rapid, and the mercury soon remained free.

Sulphate of

I was inclined to believe, that one reason why magnesia was less easy to metallize than the other alkaline earths was its insolubility in water, which would prevent it from being presented in the nascent state, detached from its solution at the negative surface. On this idea I tried the experiment, using moistened sulphate of magnesia, instead of the pure earth; and I found that the amalgam was much sooner obtained. Here the magnesia was attracted from the sulphuric acid, and probably deoxigenated and combined with the quicksilver at the same instant.

magnesia answered better than the earth.

The amalgams of the other bases of the alkaline earths could, I found, be obtained in the same manner from their saline compounds.

Salts of the other earths succeeded.

I tried in this way very successfully muriate and sulphate of lime, the muriate of strontites and of barytes, and nitrate of barytes. The earths, separated at the deoxigenating surface, there seemed instantly to undergo decomposition, and, seized upon by the mercury, were in some measure defended from the action of air, and from the contact of water, and preserved by their strong attraction for this metal.

III. *Attempts to procure the Metals of the alkaline Earths; and on their Properties.*

To procure quantities of amalgams sufficient for distillation, I combined the methods I had before employed, with those of Messrs Berzelius and Pontin.

Trial to procure the amalgams in larger quantities.

The earths were slightly moistened, and mixed with one third of red oxide of mercury; the mixture was placed on a plate of platina; a cavity was made in the upper part of it to receive a globule of mercury, of from 50 to 60 grains in the weight, the whole was covered by a film of naphtha, and the plate was made positive, and the mercury negative, by a proper communication with the battery of five hundred.

The amalgams obtained in this way were distilled in tubes of plate glass, or in some cases in tubes of common glass. These tubes were bent in the middle, and the extremities were enlarged, and rendered globular by blowing, so as to serve the purposes of a retort and receiver.

The amalgams distilled.

The tube, after the amalgam had been introduced, was filled with naphtha, which was afterward expelled by boiling, through a small orifice in the end corresponding to the receiver, which was hermetically sealed when the tube contained nothing but the vapour of naphtha, and the amalgam.

Part of the mercury easily distilled off,

I found immediately, that the mercury rose pure by distillation from the amalgam, and it was very easy to separate a part of it; but to obtain a complete decomposition was very difficult.

but the whole only with great difficulty,

For this nearly a red heat was required, and at a red heat the bases of the earths instantly acted upon the glass, and became oxigenated. When the tube was large in proportion to the quantity of amalgam, the vapour of the naphtha furnished oxigen sufficient to destroy part of the bases: and when a small tube was employed, it was difficult to heat the part used as a retort sufficient to drive off the whole of the mercury from the basis, without raising too highly the temperature of the part serving for the receiver, so as to burst the tube [*].

If at all.

In consequence of these difficulties, in a multitude of trials, I obtained only a very few successful results, and in no case could I be absolutely certain, that there was not a minute portion of mercury still in combination with the metals of the earths.

Base of barytes.

In the best result that I obtained from the distillation of the amalgam of barytes, the residuum appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness, in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis, in the first degree of oxigenation [†].

When

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[*] When the quantity of the amalgam was about fifty or sixty grains, I found that the tube could not be conveniently less than one sixth of an inch in diameter, and of the capacity of about half a cubic inch.

Bases of the earths probably the most powerful means of detecting oxigen.

[†] From this fact, compared with other facts that have been stated, p. 369, it may be conjectured, that the basis of barytes has a higher affinity for oxigen than sodium; and hence probably the bases of the earths will be more powerful instruments for detecting oxigen, than the bases of the alkalis.

I have

When exposed to air, it rapidly tarnished, and fell into a white powder, which was barytes. When this process was conducted in a small portion of air, the oxigen was found absorbed,

Base of potash applied to muriatic acid gas, fluoric acid gas, and boracic acid.

I have tried a number of experiments on the action of potassium on bodies supposed simple and on the undecompounded acids. From the affinity of the metal for oxigen, and of the acid for the substance formed, I had entertained the greatest hopes of success. It would be inconsistent with the object of this paper to enter into a full detail of the methods of operation; I hope to be able to state them fully to the Society at a future time, when they shall be elucidated by farther researches; I shall now merely mention the general results, to show that I have not been tardy in employing the means which were in my power, towards effecting these important objects.

When potassium was heated in muriatic acid gas, as dry as it could be obtained by common chemical means, there was a violent chemical action with ignition; and when the potassium was in sufficient quantity, the muriatic acid gas wholly disappeared, and from one third to one fourth of its volume of hidrogen was evolved, and muriate of potash was formed.

On fluoric acid gas, which had been in contact with glass, the potassium produced a similar effect; but the quantity of hidrogen generated was only one sixth or one seventh of the volume of gas, and a white mass was formed, which principally consisted of fluate of potash and silex, but which emitted fumes of fluoric acid when exposed to air.

When boracic acid, prepared in the usual manner, that had been ignited, was heated in a gold tube with potassium, a very minute quantity of gas only was liberated, which was hidrogen, mixed with nitrogen, (the last probably from the common air in the tube); borate of potash was formed, and a black substance, which became white by exposure to air.

The results not conclusive.

In all these instances there is great reason to believe that the hidrogen was produced from the water adhering to the acids; and the different proportions of it in the different cases are a strong proof of this opinion. Admitting this idea, it seems, that muriatic acid gas must contain at least one eighth or one tenth of its weight of water; and that the water oxigenates in the experiment a quantity of potassium, sufficient to absorb the whole of the acid.

In the cases of fluoric and boracic acids, there is probably a decomposition of these bodies; the black substance produced from the boracic acid is similar to that which I had obtained from it by electricity. The quantities that I have operated upon have been as yet absorbed, and the nitrogen unaltered; when a portion of it was introduced into water, it acted upon it with great violence and sunk to the bottom, producing in it barytes; and hidrogen was generated. The quantities in which I obtained it were too minute for me to be able to examine correctly either its physical or chemical properties. It sunk rapidly in water, and even in sulphuric acid, though surrounded by globules of hidrogen, equal to two or three times its volume; from which it seems probable, that it cannot be less than four or five times as heavy as water. It flattened by pressure, but required a considerable force for this effect.

Base of strontia.

The metal from strontites sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxidation.

Base of lime.

The metal from lime I have never been able to examine exposed to air or under naphtha. In the case in which I was able to distil the quicksilver from it to the greatest extent, the tube unfortunately broke, while warm, and at the moment that the air entered, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quicklime.

Base of magnesia.

The metal from magnesia seemed to act upon the glass, even before the whole of the quicksilver was distilled from it. In an experiment in which I stopped the process before the mercury was entirely driven off, it appeared as a solid, having the same whiteness and lustre as the other metals of

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yet too small, to enable me to separate and examine the products, and till this is done, no ultimate conclusion can be drawn.

The action of potassium upon muriatic acid gas indicates a much larger quantity of water in this substance, than the action of electricity in Dr. Henry's elaborate experiments; but in the one instance the acid enters into a solid salt, and in the other it remains aeriform; and the difficulty of decomposition by electricity must increase in proportion as the quantity of water diminishes, so that at the apparent maximum of electrical effect, there is no reason to suppose the gas free from water.

Those persons who have supposed hidrogen to be the basis of muriatic acid may, perhaps, give another solution of the phenomena, and consider the experiment I have detailed as a proof of this opinion.

the earths. It sunk rapidly in water, though surrounded by globules of gas producing magnesia, and quickly changed in air, becoming covered with a white crust, and falling into a fine powder, which proved to be magnesia.

Amalgams of the metals of the earths.

In several cases in which amalgams of the metals of the earths, containing only a small quantity of mercury, were obtained, I exposed them to air on a delicate balance, and always found, that, during the conversion of metal into earth, there was a considerable increase of weight.

Attempts to ascertain the proportion of base.

I endeavoured to ascertain the proportions of oxigen and basis in barytes and strontites, by heating amalgams of them in tubes filled with oxigen, but without success. I satisfied myself, however, that when the metals of the earths were burned in a small quantity of air they absorbed oxigen, gained weight in the process, and were in the highly caustic or unslacked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids.

The evidence for the compostion of the alkaline earths is then of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar, the inflammable matters in all cases separating at the negative surface in the Voltaic circuit, and the oxigen at the positive surface.

New names.

These new substances will demand names; and on the same principles as I have named the bases of the fixed alkalis potassium and sodium, I shall venture to denominate the metals from the alkaline earths barium, strontium, calcium, and magnium; the last of these words is undoubtedly objectionable, but magnesium * has been already applied to metallic manganese, and would consequently have been an equivocal term.

IV. *Inquiries relative to the Decomposition of Alumine, Silex, Zircone, and Glucine.*

Alumine and silex tried as the other earths.

I tried the methods of electrization and combination with quicksilver, and the common metals, by which I had succeeded in decomposing the alkaline earths, on alumine and silex; but without gaining distinct evidences of their having undergone any change in the processes.

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* Bergman. Opusc. tom. ii, p. 200.

Obliged to seek for other means of acting upon them, it was necessary to consider minutely their relations to other bodies, and to search for analogies, by which the principles of research might be guided.

Nearly indifferent to the two electricities.

Alumine very slowly finds its point of rest at the negative pole, in the electrical circuit; but silex, even when diffused in its gelatinous state through water, rests indifferently at the negative or positive poles.

Analogous to insoluble neutral salts,

or saturated oxides.

From this indifference to positive and negative elecrical attractions, following the general order of facts, it might be inferred, that if these bodies be compounds, the electrical energies of their elements are nearly in equilibrium; and that their state is either analogous to that of insoluble neutral salts, or of oxides nearly saturated with oxigen.

The combinations of silex and alumine with acids and alkalis, as well as their electrical powers, were not inconsistent with either of these ideas; for in some respects they resemble in physical characters fluate and phosphate of lime, as much as in others they approach to the oxides of zinc and tin.

Experiments to resolve silex, if neutrosaline.

On the idea that silex might be an insoluble neutrosaline compound, containing an unknown acid or earth, or both, and capable of being resolved into its secondary elements, in the same manner as sulphate of barytes, or fluate of lime, I made the following experiments.

Exposed to electricity in water.

Two gold cones*, connected by moistened amianthus, were filled with pure water, and placed in the electrical circuit, a small quantity of carefully prepared and well washed silex was introduced into the positive cone; the action was kept up from a battery of two hundred plates, for some hours, till nearly half of the fluid in each cone was exhausted; the remainders were examined; the fluid in the cone containing the silex was strongly acid: that in the opposite cone was strongly alkaline; the two fluids were passed through bibulous paper, and mixed together, when a precipitate fell down, which proved to be silex.

One vessel acid, the other alkaline, and silex dissolved.

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* The same as those described in Phil. Trans. 1807, p. 6; or Journal, vol. xviii, p. 325.

On

It might be supposed, that the silex was decomposed, and then recomposed,

On the first view of the subject, it appeared probable, that this silex had been formed by the union of the acid and the alkaline matter in the two cones, and that the experiment demonstrated a decomposition and recomposition of silex; but before such a conclusion could be made, many points were to be determined.

It was possible, that the acid might be nitric acid, produced as in other electrical experiments of a similar nature, and that this acid might have dissolved silex, which was precipitated by the alkaline matter at the other pole, which might be either potash used for dissolving the silex, which had adhered to it, notwithstanding the processes of lixiviation in acids, or ammonia produced in consequence of the presence of the atmosphere; or if potash was present, it was likewise possible, that the silex might have been carried over in solution, with this alkali, from the positive to the negative surface.

but this not the fact.

Minute experiments were instituted and completed in the same manner as those detailed in the Philosophical Transactions for 1807, p. 7 *, which soon proved, that there was no reason to suppose, that the silex had been changed in these experiments.

The acid was nitric.

Alkali not from the silex.

The acid proved to be nitric acid, which under the electrical action seemed to have dissolved the silex; the alkali turned out to be principally fixed alkali; and that it was merely an accidental ingredient, and not a constituent of the silex, appeared from this circumstance, that when the same portion of silex was long electrified, by degrees it lost its power of affording the substance in question †.

This

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* Journal, vol. xviii, p. 325.

Common chemical methods of separating bodies imperfect.

† If silex, that has been carefully washed, after precipitation by muriatic acid from liquor silicum, be moistened, and acted on by mercury negatively electrified, the mercury soon contains a notable quantity of potassium. Well washed alumine, that has been precipitated from alum by carbonate of soda, affords by the same treatment sodium and potassium, so that the powers of electrochemical analysis are continually demonstrating the imperfection of the common chemical methods of separating bodies from each other. The purest boracic acid, which can be obtained from borax by chemical decomposition, by electrical analysis is shown to contain both soda, and the decomposing acid employed in the process; and

hence

Treated as inflammables saturated with oxigen.

This result having taken place, the same plan of operation was not pursued with respect to alumine, which resembles a saline compound less than silex; and the method which I now adopted of acting upon these bodies was on the supposition of their being inflammable substances so highly saturated with oxigen as to possess little or no positive electricity.

Alumine and silex have both a strong affinity for potash and soda; now supposing them to be oxides, it was reasonable to conclude, that the oxigen, both in the alkalis and the earths, must be passive as to this power, which must consequently be referred to their bases, and on this notion it was possible, that it might be made to assist their decomposition by electricity.

Silex 1 p. potash 6 p. kept in fusion in a platina crucible and electrified.

After this reasoning, I fused a mixture of one part of silex, and six of potash in a platina crucible, and preserved the mixture fluid, and in ignition, over a fire of charcoal; the crucible was rendered positive from the battery of five hundred, and a rod of platina, rendered negative, was brought into contact with the alkaline menstruum. At the moment of contact there was a most intense light; when the rod was plunged into the liquid an effervescence took place, and globules, which burnt with a brilliant flame, rose to the surface, and swam upon it in a state of combustion. In a few minutes, when the mixture was cool, the platina bar was removed: after as much as possible of the alkali and silex had been detached from it by a knife, there remained brilliant metallic scales round it, which instantly became covered with a white crust in the air, and some of which inflamed spontaneously. The platina appeared much corroded, and of a darker tint than belongs to the pure metal. When it was plunged into water it strongly effervesced: the fluid that came from it was alkaline; when a few drops of muriatic acid were added to the solution, a white cloudiness occurred, which various trials demonstrated to depend upon the presence of silex.

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hence the experiment on the action of the boracic acid and potassium, page 375, may possibly be explained without assuming its decomposition.

A similar

Alumine and potash treated in the same manner.

A similar mixture of potash and alumine was experimented upon in the same manner, and the results were perfectly analogous; there adhered to the rod of platina a film of a metallic substance, which rapidly decomposed water, and afforded a solution which deposited alumine by the action of an acid.

The metal could not be obtained separate.

I tried several forms of this experiment, with the hopes of being able to obtain a sufficient quantity of the metallic matter from the platina, so as to examine it in a separate state; but I was not successful. It was always in superficial scales, which oxidated, becoming white and alkaline, before it could be detached in the air; it instantly burnt when heated, and could not be fused under naphtha or oil.

Experiments with alumine and soda, and zircone and soda.

I tried similar experiments with mixtures of soda and alumine, and soda and zircone, and used iron as the negatively electrified metal. In all these cases, during the whole process of electrization, abundance of globules, which swam in a state of inflammation on the fused mass, were produced. And in the mixture, when cooled, small laminæ of metal were found of the colour of lead, and less fusible than sodium, which adhered to the iron; they acted violently upon water, and produced soda and a white powder, but in quantities too small to be minutely examined.

Trials to obtain an alloy by treating as potash unsuccessful.

I endeavoured to procure an alloy of potassium, and the bases of the earths, from mixtures of potash, silex, and alumine, fused by electricity, and acted on by the positive and negative surfaces in the same manner as pure potash, in experiments for the decomposition of that substance; but I obtained no good results. When the earths were in quantities equal to one fourth or one fifth of the alkali, they rendered it so highly nonconducting, that it was not easy to affect it by electricity, and when they were in very minute portions, the substance produced had the characters of pure potassium.

Potassium heated with silex and alumine in vapour of naphtha.

I heated small globules of potassium, in contact with silex and alumine, in tubes of plate glass filled with the vapour of naphtha: the potassium seemed to act at the same time upon the glass and the earths, and a grayish opaque mass, not possessed of metallic splendour, was obtained, which effervesced in water, depositing white clouds. Here it was possible that the potash had been converted wholly or partly into protoxide, by its action upon the earths; but as no globule was obtained, and as the plate glass alone might have produced the effect, no decided inference of the decomposition of the earths can be drawn from the process.

Inconclusive.

I shall now mention the last trials that I made with respect to this object.

Amalgam of potassium electrified with silex,

Potassium, amalgamated with about one third of mercury, was electrified negatively under naphtha, in contact with silex very slightly moistened, by the power of five hundred; after an hour the result was examined. The potassium was made to decompose water, and the alkali formed neutralized by acetous acid; a white matter, having all the appearance of silex precipitated, but in quantity too small for accurate examination.

alumine, glucine,

I tried the same method of action upon alumine and glucine, and obtained a cloudiness, more distinct than in the case of silex, by the action of an acid upon the solution obtained from the amalgam.

and zircone.

Zircone exposed in the same manner to the action of electricity, and the attraction of potassium, furnished still more satisfactory results, for a white and fine powder, soluble in sulphuric acid, and which was precipitated from sulphuric acid by ammonia, separated from the amalgam that had been obtained by the action of water.

They all appear to be metallic oxides,

From the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude that alumine, zircone, glucine, and silex are, like the alkaline earths, metallic oxides, for on no other supposition is it easy to explain the phenomena, that have been detailed.

but the evidence not so strict.

The evidences of decomposition and composition are not however of the same strict nature as those that belong to the fixed alkalis and alkaline earths; for it is possible, that in the experiments in which the silex, alumine, and zircone appeared to separate during the oxidation of potassium and sodium, their bases might not actually have been in combination with them, but the earths themselves in union with the metals of the alkalis, or in mere mechanical mixture. And out of an immense number of experiments, which I made

e of the kind last detailed, a very few only gave distinct
ations of the production of any earthy matter; and in
s when earthy matter did appear, the quantity was such,
ndered it impossible to decide on the species.

ad I been so fortunate as to have obtained more certain
ences on this subject, and to have procured the metallic
tances I was in search of, I should have proposed for
the names of silicium, alumium, zirconium, and glu-
a.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian Society.

T the meeting of the Wernerian Natural History Society
he 12th of November, the Rev. Andrew Jameson, mi-
er of St. Mungo, Dumfriesshire, read a paper entitled
ervations on Meteorological Tables, with a description
new Anemometer. After some general observations
the importance of meteorological observations, and on
merits and defects of registers of the weather, &c., he
ted out what he considered to be the best form of a me-
ological journal, and then described the external form
internal structure of an extensive and complete meteor-
ical observatory, and enumerated about twenty differ-
instruments, which ought to find a place in every esta-
ment of that kind. He remarked, that a daily exami-
on of the changes which take place in those instruments,
ed with a careful record of the external appearances in
atmosphere, will afford a constant and fascinating em-
ment to the most zealous observer, and will in time en-
us to form a just theory of meteors; to prognosticate
considerable accuracy the nature of the coming wea-
; and, lastly, enable us to ascertain the climate of dif-
nt countries, with the view of determining the influence
erts on organic bodies. He next described an Anemo-
er, which, by a very simple and ingenious arrangement
parts, will enable the most common observer to ascer-
the velocity of the wind with perfect accuracy.

Meteorological observations.

Their use.

t the same meeting, the Rev. John Fleming, F.A.S.
ister of Bressay in Shetland, who has been for some
time

Mineralogy of the Shetland isles.

Mineralogy of the Shetland isles.

time past employed in examining the mineralogy of those remote islands, communicated to the Society an interesting account of the geognostic relations of the rocks in the islands of Unst and Papa Stour; in the course of which he gave answers to the queries formerly published regarding the serpentine and sandstone of Shetland. After a general account of the position, extent and external appearance of the island of Unst, he next described the different rocks of which it is composed, in the order of their relative antiquity, and remarked, that their general direction is from S. W. to N. E. The rocks are gneiss, mica-slate, clay-slate, limestone, hornblende-rock, potstone, and serpentine.—The *gneiss* in some places appeared to alternate with the oldest mica-slate, and in others to contain beds of hornblende-rock. The *mica-slate*, which is the most abundant rock in the island, is traversed by numerous contemporaneous veins of quartz, and also of feldspar, and passes distinctly into clay-slate. It contains beds of hornblende-rock and of limestone. The *clay-slate* occurs but sparingly in this island. The *potstone* usually accompanies the serpentine. The *serpentine* occurs in great abundance, *in beds*, in the oldest clay-slate and newest mica-slate, and hence must be referred to the oldest or first serpentine formation of Werner. Mr. Fleming is also inclined to believe, that the serpentine of the neighbouring island of Fetlar belongs to the same formation. The Island of Papa Stour, situate on the west coast of the Mainland, (as the largest of the islands is called), contains no primitive rocks; on the contrary, it appears to be entirely composed of floetz rocks. These are, conglomerate, greenstone, claystone, porphyritic stone, hornstone, and sandstone. The sandstone, as appears from observations made in this island and other parts of Shetland, would seem to belong to the oldest coal-formation. The claystone, conglomerate, porphyritic stone, greenstone, and hornstone (probably clinkstone) rest on the sandstone. In some places Mr. Fleming observed the greenstone alternating with the sandstone, hence he properly concludes, that they belong to the same formation. In no place, however, did he observe any of the other rocks alternating with the sandstone; and therefore the formation to which they belong remains still somewhat problematical.

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A

JOURNAL

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VOL. XXII.

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BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, the Rev. E. J. Burrow, A. M.; Sir Thomas Clarges, Bart.; P. Barlow, Esq.; Mr. John Farey; Mr. William Skrimshire, jun.; Dr. Thomas Stewart Traill; A Correspondent; Richard Greene, Esq. P. R. M. S. E.; Mr. B. Cook; Mr. James Scott; G. K. M.; Philochemicus; Mr. de Luc; E. F. G. H.; S. N.; Sir George Cayley, Bart; T.; Mr. Charles Hayter; Mr. W. Weldon; W. Saint, Esq.; Arthur Aikin, Esq.; Thomas Young, M. D. F. R. S.; John Gough, Esq.; J R. Gowen, Esq.; C. O. T.

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The Engravings consist of 1. An improved Goniometer, by the Rev. William Burrows: 2. Views of the late Comet, by Dr. Herschel: 3. Crystals of the Diopside, a new Species of Mineral: 4. Mr. Christopher Towill's Life Boat: 5. Delphinus Melas, a non descript of the Order Cete: 6. Dr. W. Henry's Apparatus for the Analysis of the Compound inflammable Gasses by slow Combustion: 7. Messrs. Allen and Pepys's Apparatus for showing the Changes produced in Air by Respiration. 8. View of Portmoon: 9. View of Pleskin, on the N. W. Side of Bengore Promontory: 10. Sir George Cayley's Plan for a Theatre: 11. The Chinese Method of propagating Fruit Trees: 12. Mr. Broad's Instrument for measuring standing Timber: 13. Mr. Le Caan's improved Tram Plates for Rail Roads: 14. Guyton Morveau's permanent Apparatus for destroying Contagion.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY, 1809.

ARTICLE I.

An account of a Goniometer invented by the Rev. E. J. Burrow, *A. M., Fellow of Magdalen College, Cambridge.*

To Mr. NICHOLSON.

SIR, *Walthamstow, Dec. 6th,* 1808.

New goniometer

SHOULD you think the following account, accompanied by a drawing, of a goniometer made by Mr. Harris of Holborn, under my directions, upon, I believe, a new principle, at all worthy of a place in your Journal, by inserting it you will oblige

Your obedient servant,

E. J. BURROW.

described.

B G, Plate I, fig. 1, is a steel bar of about $\frac{1}{10}$ of an inch square, chamfered off to the point B. On B G is taken exactly an inch, B A, and A is made the centre of motion f the legs D E and *d e*, of which D E is also brought to a oint at D to correspond with B. To the other end of these legs is attached by the pin F, a movable quadrant, passing through the bar B G at C, and graduated to read towards the side of the shorter leg, *d e*. The handle G H is made to project on the opposite side to that on which the legs move, that it may not interfere with the use of a brass degree divided into minutes, to be attached to the centre A upon longer radii. The leg A B is divided accurately into tenths, and the two nearest the point B into twentieths, and these again into fortieths. The whole instrument is about four inches and a half long.

Method of using it.

Now if the crystal, the angle of which is to be measured, be detached, it is obvious, that, if an acute one, by applying it to the angle B A D the vertical and equal angle E A C will be given on the quadrant: if obtuse, by applying it to the angle E and B we obtain its supplement E A C.— But if the crystal be imbedded in the matrix, so that with the common goniometers having the movable vertex it is difficult to procure a mechanical measurement of the protruding angle, we take any distance from the vertex of the crystal measured upon the scale of tenths, &c.; and placing one point of the instrument, *(ex. gr.)* on x (fig. 2) and the other on z, *so as to make the triangle $x\,y\,z$ isoscelles*, we get upon the quadrant the angle which the base $x\,z$ subtends at the radius one inch; and we have the side $x\,y$ measured. As, therefore, when the object is given, the angle it subtends, and $\frac{1.}{\text{rad.}}$, we have the proportion xy : A B :: angle found : angle sought. And as the whole radius is one inch divided into tenths, this proportion is easily made in the head.

Inclinations of strata ascertained by it.

The short bar is added for the better carrying of the quadrant, and renders the instrument useful in geological observations, to ascertain the inclinations of strata. This is done by hanging a small plummet to a hook at A, and having made the angle which it forms at B C a right angle, and moving the outer bars, till their upper line coincides with the stratum, the angle of inclination will be given on the quadrant.

II.

II.

Observations of a Comet, made with a view to investigate its Magnitude, and the Nature of its Illumination. By WILLIAM HERSCHEL, *LL. D. F. R. S*[*].

Comet when seen.

THE comet, which we have lately observed, was pointed out to me by Mr. Piggot, who discovered it at Bath the 28th of September, and the first time I had an opportunity of examining it was the 4th of October, when its brightness to the naked eye gave me great hopes to find it of a different construction from many I have seen before, in which no solid body could be discovered with any of my telescopes.

Examined with respect to its physical condition.

In the following observations, my attention has been directed to such phenomena only, as were likely to give us some information relating to the physical condition of the comet, it will therefore not be expected, that I should give an account of its motion, which I was well assured would be most accurately ascertained at the Royal Observatory at Greenwich.

Terms relating to comets frequently misapplied.

The different parts of a comet have been generally expressed by terms that may be liable to misapprehension, such as the head, the tail, the coma, and the nucleus; for in reading what some authors say of the head, when they speak of the size of the comet, it is evident that they take it for what is often called the nucleus. The truth is, that inferior telescopes, which cannot show the real nucleus, will give a certain magnitude of the comet, which may be called its head; it includes all the very bright surrounding light; nor is the name of the head badly applied, if we keep it to this meaning; and since, with proper restriction, the terms which have been used may be retained, I shall give a short account of my observations of the comet, as they relate to the above-mentioned particulars, namely, the nucleus, the head, the coma, and the tail, without regarding the order of the time when they were made. The date of each

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[*] Philosophical Transactions for 1808, Part II, p. 145.

observation, however, will be added, that any perso may hereafter be in possession of more accurate elem the comet's orbit, than those which I have at present repeat the calculations in order to obtain a more accur sult.

Of the Nucleus.

Nucleus.

From what has already been said, it will easily be stood, that, by the nucleus of the comet, I mean tha of the head which appears to be a condensed or solid and in which none of the very bright coma is include should be remarked, that from this definition it follow when the nucleus is very small, no telescope, but wh light and power in an eminent degree, will show it disti

Observations.

Nucleus has a visible disk,

Oct. 4, 1807. 10-feet reflector. The comet has cleus, the disk of which is plainly to be seen.

which is real.

Oct. 6. I examined the disk of the comet with a set of diaphragms, such as described in a former pa in order to see whether any part of it were spurious when the exterior light was excluded, so far from appe larger, as would have been the case with a spurious d appeared rather diminished for want of light; nor w diameter lessened when I used only the outside rays mirror. The visible disk of the comet therefore is one.

Oct. 4. I viewed the comet with different magn powers, but found that its light was not sufficiently i to bear very high ones. As far as 200 and 300, my 1 reflector acted very well, but with 400 and 500 there w thing gained, because the exertion of a power depe on the quantity of light was obstructed†, which I foun here of greater consequence than the increase of magn

Illumination of the Nucleus.

Apparently

Oct. 4, 6h. 15′ The nucleus is apparently round

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* See Phil. Trans. for 1805, page 53. Use of the Criterion.

† See Phil. Trans. for 1800, p. 78.

round and uniformly bright.

equally bright all over its disk. I attended particularly to its roundness.

Oct. 18. The nucleus is not only round, but also every where of equal brightness.

Oct. 19. I see the nucleus again, perfectly round, well defined, and equally luminous. Its brilliant colour in my 10 feet telescope is a little tinged with red; but less so than that of Arcturus to the naked eye.

Magnitude of the Nucleus.

Its magnitude.

Oct. 26. In order to see the nucleus as small as it really is, we should look at it a long while, that the eye may gradually lose the impression of the bright coma which surrounds it. This impression will diminish gradually, and when the eye has got the better of it, the nucleus will then be seen most distinctly, and of a determined magnitude.

Diameter less than 3 seconds.

Oct. 4. With a 7 feet reflector I estimated the diameter of the nucleus of the comet at first to be about five seconds, but soon after I called it four, and by looking at it longer, I supposed it could not exceed three seconds.

Apparent disk less than that of Herschel.

Oct. 6. 10 feet reflector, power 221. The apparent disk of the comet is much less than that of the Georgian planet, which being an object I have seen so often with the same instrument, and magnifying power, this estimation from memory cannot be very erroneous.

Compared with globules of sealing wax.

Oct. 5. Micrometers for measuring very small diameters, when high magnifying powers cannot be used, being very little to be depended upon, I erected a set of sealing wax globules upon a post at 2422 inches from the object mirror of my 10 feet reflector, and viewed them with an eye glass, which gives the instrument a power of 221, this being the same which I had found last night to show the nucleus of the comet well. I kept them in their place all the day, and reviewed them from time to time, that their magnitudes might be more precisely remembered in the evening, when I intended to compare the appearance of the nucleus with them.

On examining the comet, I found the diameter of its nucleus to be certainly less than the largest of my globules, which, being ·0466 of an inch, subtended an angle of 3″·97 at the distance of the telescope in the day time.

Comparing the nucleus also with the impressions, which the view of the second and third had left in my memory, and of which the real diameters were ·0325 and ·0290 of an inch: and magnitudes at the station of the mirror 2″·77 and 2″·47, I found, that the comet was almost as large as the second, and a little larger than the third.

Oct. 18. The nucleus is less than the globule which subtends 2″·77.

Oct. 19. The air being uncommonly clear, I saw the comet 40 minutes after five, and being now at a considerable altitude, I examined it with 289, and having but very lately reviewed my globules, I judged its diameter to be not only less than my second globule, but also less than the third: that is, less than 2″·47.

Oct. 6. The 20-feet reflector, notwithstanding its great light, does not show the nucleus of the comet larger than the 10 feet, with an equal magnifier, makes it.

Oct. 28. My large 10 feet telescope, with the mirror of 24 inches in diameter, does not increase the size of the nucleus.

Compared with the 3d satellite of Jupiter.

Oct. 6. Being fully aware of the objections, that may be made against the method of comparing the magnitude of the nucleus of the comet with objects that cannot be seen together, I had recourse to the satellites of Jupiter for a more decisive result, and with my 7 feet telescope, power 202, I viewed the disk of the third satellite and of the nucleus of the comet alternately. They were both already too low to be seen very distinctly; the diameter of the nucleus however appeared to be less than twice that of the satellite.

Oct. 18. With the 10 feet reflector, and the power 221, a similar estimation was made; but the light of the moon would not permit a fair comparison.

Examined with a new mirror.

Oct. 19. I had prepared a new 10 feet mirror, the delicate polish of my former one having suffered a little from being exposed to damp air in nocturnal observations. This new one being uncommonly distinct, and the air also remarkably clear, I turned the telescope from the comet to Jupiter's third satellite, and saw its diameter very distinctly larger than the nucleus of the comet. I turned the telescope again to the comet, and as soon as I saw it distinctly round and well defined, I was assured that its diameter was less than that of the satellite.

6h 20′. I repeated these alternate observations, and always found the same result. The night is beautifully clear, and the moon has not yet risen to interfere with the light of the comet.

Nov. 20. With a 7 feet reflector, and power only 75, I can also see the nucleus; it is extremely small, being little more than a mere point.

Of the Head of the Comet.

Head of the comet.

When the comet is viewed with an inferior telescope, or if the magnifying power, with a pretty good one, is either much too low, or much too high, the very bright rays immediately contiguous to the nucleus will seem to belong to it, and form what may be called the head.

Oct. 19. I examined the head of the comet with an indifferent telescope, in the manner I have described, and found it apparently of the size of the planet Jupiter, when it is viewed with the same telescope and magnifying power.

With a good telescope, I saw in the centre of the head a very small well defined round point.

Nov. 20. The head of the comet is now less brilliant than it has been.

Of the Coma of the Comet.

Its coma.

The coma is the nebulous appearance surrounding the head.

Oct. 19. By the field of view of my reflector, I estimate the coma of the comet to be about 6 minutes in diameter.

Dec. 6. The extent of the coma, with a mirror of 24 inches diameter, is now about 4′ 45″.

Of the Tail of the Comet.

Its tail.

Oct, 18. 7h. With a night glass, which has a field of view of nearly 5°, I estimated the length of the tail to be 3°½; but twilight is still very strong, which may prevent my seeing the whole of it.

Nov. 20. The tail of the comet is still of a considerable length, certainly not less than 2½ degrees.

Oct. 26. The tail of the comet is considerably longer on the south-preceding, than on the north-following side.

It is not bifid, as I have seen the comet of 1769 delineated by a gentleman who had carefully observed it*.

Oct. 28. 7 feet reflector. The south-preceding side of the tail in all its length, except towards the end, is very well defined; but the north-following side is every where hazy and irregular, especially towards the end; it is also shorter than the south-preceding one.

The shape of the unequal length of the sides of the tail, when attentively viewed, is visible in a night glass, and even to the naked eye.

Oct. 31. 10 feet reflector. The tail continues to be better defined on the south-preceding than on the north-following side.

Dec. 6. The length of the tail is now reduced to about 23′ of a degree.

Of the Density of the Coma and Tail of the Comet.

Density of the coma and tail

Many authors have said, that the tails of comets are of so rare a texture, as not to affect the light of the smallest stars that are seen through them. Unwilling to take any thing upon trust, that may be brought to the test of observation, I took notice of many small stars, that were occasionally covered by the coma and the tail, and the result is as follows.

sufficient to obscure the stars seen through them

Oct. 26, 6h. 15′. Large 10 feet reflector, 24 inches aperture. A small star within the coma is equally faint with two other stars that are on the north-following side of the comet, but without the coma.

7h. 30′. The coma being partly removed from the star, it is now brighter than it was before.

Oct. 31, 6h. 5′. 10 feet reflector. A star in the tail of the comet, which we will call *a*, is much less bright than two others, *b* and *c*, without the tail.

Two other stars, *d* and *e*, towards the south of *b* and *c*, are in the following skirts of the tail, and are extremely faint.

7h. 20′. The star *e* is now considerably bright, the tail having left it, while *d*, which is rather more involved than it was before, is hardly to be seen.

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* Dr. Lind of Windsor.

7h. 50′. The star *a*, toward which the comet moves, is involved in denser nebulosity than before, and is grown fainter.

d is involved in brighter nebulosity than before, but being near the margin, it will soon emerge.

8h. 35′. Being still more involved, the star *a* is now hardly visible.

e is quite clear of the tail, and is a considerable star; *d* remains involved.

9h. 10′. The star *d* is also emerged, but the comet is now too low to estimate the brightness of stars properly.

Nov. 25, 7h. 35′. There is a star *a* within the light of the tail, near the head of the comet, equal to a star *b* situate without the tail, but near enough to be seen in the field of view with *a*. The path of the head of the comet leads towards *a*, and a more intense brightness will come upon it.

8h. 46′. The star *a* is now involved in the brightness near the head of the comet, and is no longer visible, except now and then very faintly, by occasional imperfect glimpses; but the star *b* retains its former light.

Nebulous appearance of the Comet.

Nebulous appearance of the comet.

Dec. 6. The head of the comet, viewed with a mirror of 24 inches diameter, resembles now one of those nebulæ, which in my catalogues would have been described, "a very large, brilliant, round nebula, suddenly much brighter in the middle."

Dec. 16. 7 feet reflector. The night being fine, and the moon not risen, the comet resembles "a very bright, large, irregular, round nebula, very gradually much brighter in the middle, with a faint nebulosity on the south-preceding side."

Jan. 1, 1808. 7 feet. "Very bright, very large, very gradually much brighter in the middle."

If I had not known this to be a comet, I should have added to my description of it as a nebula, that the centre of it might consist of very small stars; but this being impossible, I directed my 10 feet telescope with a high power to the comet

met, in order to ascertain the cause of this appearance; in consequence of which I perceived several small stars shining through the nebulosity of the coma.

Jan. 14. 7 feet. "Bright, pretty large, irregular round, brighter in the middle."

Feb. 2. 10 feet, 24 inch aperture. "Very bright, large, irregular round, very gradually much brighter in the middle." There is a very faint diffused nebulosity on the north preceding side; I take it to be the vanishing remains of the comet's tail.

Feb. 19. Considerably bright; about $\frac{1}{7}$ of the field = 3′ 26″ "in diameter, gradually brighter in the middle." The faint nebulosity in the place where the tail used to be still projects a little farther from the centre than in other directions.

Feb. 21. Less bright than on the 19th; nearly of the same size: gradually brighter in the middle. The nebulosity still a little projecting on the side where the tail used to be.

Result of the foregoing Observations.

General inferences.

From the observations which are now before us, we may draw some inferences, which will be of considerable importance with regard to the information they give us, not only of the size of the comet, but also of the nature of its illumination.

It is a solid body,

A visible, round, and well defined disk, shining in every part of it with equal brightness, elucidates two material circumstances; for since the nucleus of this comet, like the body of a planet, appeared in the shape of a disk, which was experimentally found to be a real one, we have good reason to believe, that it consists of some condensed or solid body, the magnitude of which may be ascertained by calculation. For instance, we have seen, that its apparent diameter, the 19th of October, 6h. 20′, was not quite so large as that of the third satellite of Jupiter. In order therefore to have some idea of the real magnitude of our comet, we may admit, that its diameter at the time of observation was about 1″, which certainly cannot be far from truth. The diameter of the 3d satellite of Jupiter, however. is known to have a permanent disk, such as may at any convenient time be measured with all the accuracy that can be used; and when the result of such a measure has given us the diameter of this satellite, it may by calculation be brought to the distance from the Earth at which, in my observation, it was compared with the diameter of the comet, and thus more accuracy, if it should be required, may be obtained. The following result of my calculation however appears to me quite sufficient for the purpose of general information. From the perihelion distance 0·647491, and the rest of the given elements of the comet, we find, that its distance from the ascending node on its orbit at the time of observation was 73° 45′ 44″; and having also the Earth's distance from the same node, and the inclination of the comet's orbit, we compute by these data the angle at the sun. Then by calculating in the next place the radius vector of the comet, and having likewise the distance of the Earth from the sun, we find by computation, that the distance of the comet from the Earth at the time of observation was 1·169192, the mean distance of the Earth being 1. Now since the disk of the comet was observed to subtend an angle of 1″, which brought to the mean distance of the Earth gives 1″·169, and since we also know that the Earth's diameter, which, according to Mr. Dalby, is 7913·2 miles*, subtends at the same distance an angle of 17″·2 we deduce from these principles the real diameter of the comet, which is 538 miles.

538 miles in diameter.

Its illumination.

Having thus investigated the magnitude of our comet, we may in the next place also apply calculation to its illumination. The observations relating to the light of the comet were made from the 4th of October to the 19th. In all which time the comet uniformly preserved the appearance of a planetary disk fully enlightened by the sun: it was every where equally bright, round, and well defined on its borders. Now as that part of the disk which was then visible to us could not possibly have a full illumination from the sun, I have calculated the phases of the comet for the

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* See Phil. Trans. for 1791, page 239. Mr. Dalby gives the two semiaxes of the Earth, from a mean of which the above diameter 7913·1682 is obtained.

4th and for the 19th, the result of which is, that on the 4th the illumination was 119° 45′ 9″, as represented in Pl. I, fig. 3, and that on the 19th it had gradually increased to 124° 22′ 40″, of which a representation is given in fig. 4. Both phases appear to me sufficiently defalcated, to prove that the comet did not shine by light reflected from the sun only; for had this been the case, the deficiency I think would have been perceived, notwithstanding the smallness of the object. Those who are acquainted with my experiments on small silver globules* will easily admit, that the same telescope, which could show the spherical form of balls, that subtended only a few tenths of a second in diameter, would surely not have represented a cometary disk as circular, if it had been as deficient as are the figures which give the calculated appearances.

indicates it to be luminous of itself.

If these remarks are well founded, we are authorised to conclude, that the body of the comet on its surface is self-luminous, from whatever cause this quality may be derived. The vivacity of the light of the comet also had a much greater resemblance to the radiance of the stars, than to the mild reflection of the sun's beams from the moon, which is an additional support of our former inference.

Which is farther proved by the stars seen through the tail.

The changes in the brightness of the small stars, when they are successively immerged in the tail or coma of the comet, or cleared from them, prove evidently, that they are sufficiently dense to obstruct the free passage of star-light. Indeed if the tail or coma were composed of particles that reflect the light of the sun, to make them visible we ought rather to expect, that the number of solid reflecting particles, required for this purpose, would entirely prevent our seeing any stars through them. But the brightness of the head, coma, and tail alone, will sufficiently account for the observed changes, if we admit, that they shine not by reflection, but by their own radiance; for a faint object projected on a bright ground, or seen through it, will certainly appear somewhat fainter, although its rays should meet with no obstruction in coming to the eye. Now, as in this case we are sure of the bright interposition of the parts of the comet, but have no knowledge of floating particles, we ought certainly not to ascribe an effect to a hypothetical cause, when the existence of one, quite sufficient to explain the phenomena, is evident.

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* See Phil. Trans. for 1805, p. 38, the 5th experiment.

Other circumstances prove the same.

If we admit, that the observed full illumination of the disk of the comet cannot be accounted for from reflection, we may draw the same conclusion, with respect to the brightness of the head, coma, and tail, from the following consideration. The observation of the 2d of February mentions, that not only the head and coma were still very bright, but that also the faint remains of the tail were still visible; but the distance of the comet from the Earth, at the time of observation, was nearly 240 millions of miles*, which proves, I think, that no light reflected from floating particles could possibly have reached the eye, without supposing the number, extent, and density of these particles far greater than what can be admitted.

My last observation of the comet, on the 21st of February, gives additional support to what has been said; for at the time of this observation, the comet was almost 2·9 times the mean distance of the sun from the Earth†. It was also nearly 2·7 from the sun‡. What chance then could rays going to the comet from the sun, at such a distance, have to be seen after reflection, by an eye placed at more than 275 millions of miles § from the comet? And yet the instant the comet made its appearance in the telescope, it struck the eye as a very conspicuous object.

Tails of comets not vapour.

The immense tails also of some comets that have been observed, and even that of the present one, the tail of which, on the 18th of October, was expanded over a space of more than 9 millions of miles‖, may be accounted for more satisfactorily, by admitting them to consist of radiant matter, such as, for instance, the aurora borealis, than when we unnecessarily ascribe their light to a reflection of the sun's illumination thrown upon vapours supposed to arise from the body of the comet.

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* 239894939.

† The sun's mean distance being 1, that of the comet was 2·89797.

‡ The comet's distance from the sun was 2·683196.

§ 275077889.

‖ 9260542.

By the gradual increase of the distance of our comet, we have seen, that it assumed the resemblance of a nebula; and it is certain, that had I met with it in one of my sweeps of the zones of the heavens, as it appeared on either of the days between the 6th of December, and the 21st of February, it would have been put down in the list I have given of nebulæ. This remark cannot but raise a suspicion, that some comets may have actually been seen under a nebulous form, and as such have been recorded in my catalogues; and were it not a task of many years labour, I should undertake a review of all my nebulæ, in order to see whether any of them were wanting, or had changed their place, which certainly would be an investigation, that might lead to very interesting conclusions.

III.

Remarks on the Diopside, a new Species in Mineralogy established by Mr. Haüy, comprising two Varieties found in the Piedmontese Alps by Mr. Bonvoisin, *and described in the Journal de Physique for May,* 1806, *under the names of Mussite and Alalite. By Mr.* Tonnelier, *Keeper of the Cabinet of Mineralogy to the Council of Mines*[*].

Discovery of new substances the best reward of the naturalist.

THE naturalist, who is led by his zeal to researches attended with much toil, feels himself well rewarded, if in his travels he be so fortunate, as to meet with substances not yet known. Such discoveries he considers as the most valuable recompense of his labours; and he deems it his duty, to publish descriptions of the new objects, with which he has enriched the field of science. Mr. Bonvoisin, a much respected natural philosopher, member of the imperial academy of Turin and of the legislative body, has recently experienced this satisfaction. Many celebrated naturalists had visited the Piedmontese Alps before him, and made known to us, among the subjects they have had opportunities of observing,

This occurred to Bonvoisin in the Piedmontese Alps.

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[*] Journal des Mines, vol. XX, p. 65.

serving, those that appeared to them most interesting either for their novelty or their rarity. The lithology of these regions, less assiduously cultivated than the other branches of natural history, appeared to the learned academician an ample field, in which science might promise itself a rich harvest. His expectations were not disappointed, and the result of his researches, the merit of which is enhanced by the difficulties he had to overcome, is an account of his travels, the speedy appearance of which he gives us reason to hope. In the mean time we have to express our thanks to Mr. Bonvoisin for having made us acquainted with the principal substances he has collected in his mineralogical tour. Of these a very ample description is given in the Journal de Physique for May 1806, but my attention is confined at present to two of them, to which he has given the provisional names of mussite and alalite.

Two of the new substances found by him particularly noticed.

A few days ago I was present at a meeting, at which Mr. Haüy exhibited to several of his pupils the new substances, which he purposes to describe in his course of lectures this year, and among others those which Mr. Bonvoisin has sent from Piedmont to Mr. Fourcroy, who has destined the most remarkable for the gallery of the Museum of Natural History*. Among these substances two particularly engaged my attention, which the celebrated professor of mineralogy informed us he had been led by his observations to unite in one species, the essential characters of which differ completely from those that distinguish all the known species, notwithstanding their appearance seems to indicate, that they should be separated. The constant occupation, which the approaching publication of the second edition of his Treatise on Natural Philosophy imposes on Mr. Haüy, not allowing him to publish the results of his examination of the substances in question†, I requested his permission, to take this task upon myself, and drew up an article on the subject for the Journal des Mines, persuaded, that these new observations could not fail to be interesting to its readers. This I now purpose to perform, tracing here the principal characters of the two substances discovered by Mr. Bonvoisin.

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* Mr. Haüy has since given a description of the diopside, which constitutes the subject of the present paper, in the public lecture on mineralogy, which he gave at the Museum of Natural History on the 12th of July this year.

† With respect to the diopside Mr. Haüy has followed his usual custom of committing to writing the new observations he delivers to his pupils, and depositing a copy in the library of the Museum, which every one is at liberty to transcribe after the lecture.

Characters of the diopside.

Characters of the new species diopside.

Its specific gravity is 3·2374. It does not scratch glass, or very slightly; but scratches fluate of lime. Before the blowpipe it fuses into a glass of the same greenish colour as the mass itself. Its primitive form is a right angled quadrangular prism, Pl. I, fig. 5, with oblique bases, the angle of incidence between the diagonal drawn from A to O and the edge H is 107° 8′. The prism is subdivisible by very clean sections in the direction of the diagonals of its bases[‡]. The divisions parallel to the bases are in general very clean; those that answer to the sides *m m* are less easy to obtain.

I. *Determinate varieties of form.*

Determinate varieties of form.

The two principal varieties pointed out by Mr. Haüy are,

Primitive.

1st variety, Primitive diopside: a variety of the mussite of Mr. Bonvoisin. Fig. 5.

Care must be taken, not to confound the joints exposed on breaking the crystals exhibiting this variety with its natural bases.

Didodecaedral

2d variety, Didodecaedral diopside, fig. 6. A twelve-sided prism, terminated at each extremity by six faces, situate two and two, one above the other.

The angle of incidence between M and M is 90°
r and M 135°
s and M 135°
p and r 107° 8′
n and r 137° 12′
l and s 145° 53′
l and p 124° 7′
t and s 161° 16′
x and r 153° 26′
l and M 134° 86′
l and the surface contiguous to S, behind the crystal, 117° 55′

This

[‡] If from the point O a perpendicular be let fall on the side opposite to the edge H, the ratio between this perpendicular and the part intercepted

Mr. Burron's Goniometer

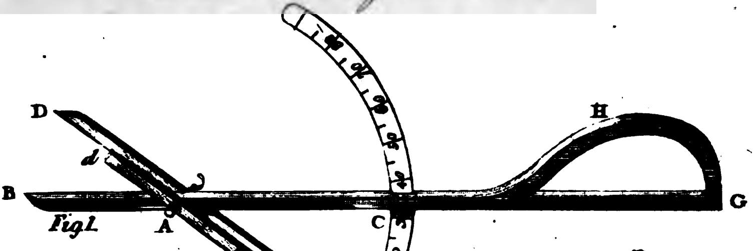

Fig. 1.

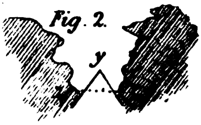

Fig. 2.

Comet of 1807

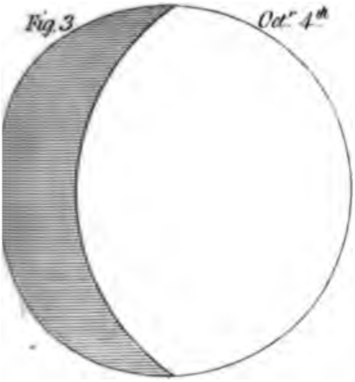

Fig. 3. Oct.r 4th

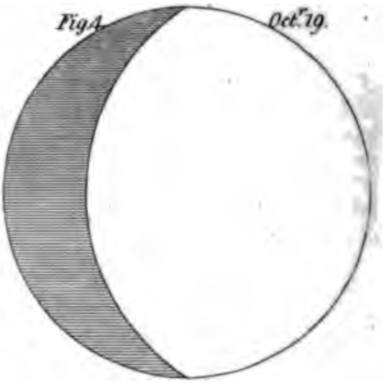

Fig. 4. Oct.r 19.

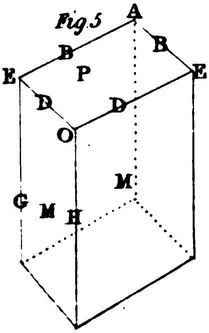

Fig. 5

Diopside

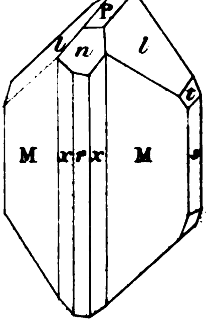

Fig. 6.

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TILDEN FOUNDATIONS
R L

The angles of a crystal must be accurately ascertained,

This figure is analogous to that described in the Journal de Physique, May 1806, p. 430, as belonging to the regular crystals of alalite. The author of the description was aware, that, to give an accurate idea of a crystal, the number and position of the faces will not be sufficient, but that the quantities of the angles they form with each other must be assigned; so that if he had contented himself with mentioning the number of sides that form the prism; and the faces that terminate it, he would have given but a very imperfect idea of the crystalline form he wished to make known, as the description would apply to several different figures, on which account it would be of little use. However, having had recourse to the goniometer alone, the measures of which are merely approximations, without the assistance of calculation, he has not sufficiently guarded against the danger of giving angles inconsistent with the principles of geometry, to which the crystallographer exposes himself, who neglects the resources of algebra. Thus the faces M M being acknowledged to form a right angle with each other, and the angles of incidence between *l* M, and *l s*, or the edge that sometimes occupies the place of the latter, being given, the third, or inclination of the face adjacent to *s*, follows necessarily. Now on calculating this from the other data in the Journal de Physique, we find, that the result of the mechanical measurement is erroneous by several degrees.

which cannot be done by measurement merely.

Romé de l'Isle sometimes mistaken from trusting to the goniometer.

The descriptions of Romé-de-l'Isle, though in general accurate, sometimes exhibit instances of these contradictions between the values of the angles of the same crystal. This learned gentleman, for instance, after having given 105° as the great angle of the rhombus of inverted carbonate of lime, which he called muriatic calcareous spar, gives 115° for the great angle of the principle section, or that which passes through the oblique diagonals of two opposite faces, and the intermediate edges*. Now if we take the first angle as given, we shall be led by calculation to 109°4 for the second, which makes a difference of more than 5° degrees

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cepted by it toward the point A will be $\sqrt{21} : \sqrt{2}$; and this intercepted part will be to either of the edges, G or H, as 1 to 5.

[*] *Cristallographie*, vol. 1, p. 520.

from the angle determined by observation. This value is too far from the truth, not to lead us to a suspicion of some irregularities in the crystal, which that celebrated crystallographer examined. It is equally certain, that calculation would have acquainted him with his mistake, in affording him a sure method of correcting his observation. I might mention other instances, if I were not afraid of wandering from my subject. That which I have adduced is sufficient, to show the justice of the remarks on the manner of describing crystals may by Mr. Haüy in the Annals of the Museum of Natural History. This gentleman has there shown, that the descriptions of a crystal, to be precise, must indicate the angles as determined by the concurrence of common geometry with that founded on the structure of minerals. By following this method, the only one compatible with strict accuracy, we shall be certain, that the angles will always agree with each other. They will be as so many limits, to which the observer may come sufficiently near with the goniometer, to be able to refer a crystal to the species or variety, the characters of which it bears. This is all we can expect from this instrument, however nicely it may be executed, and however skilful the hand that employs it.

Geometrical calculation therefore necessary

assisted by the laws of crystallization.

The diopside a distinct species.

To return to the diopside. Though this substance has never yet been subjected to analysis, Mr. Haüy does not hesitate to consider it as a species, that should occupy a distinct place in the system*. The few characters I have given are sufficient, to demonstrate this assertion, because they occupy the first rank among those that are truly specific. The primitive form obtained by mechanical division differs from all other known forms. Far from having the character of a limitation, it is remarkable for a singularity, that no other species has yet presented. This consists in the double appearance it exhibits, one in its prism, which is a foursided rectangle,

The diopside has some analogy to the pyroxene.

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* The place assigned to the diopside is immediately after the pyroxene, the primitive form of which bears some analogy to it. In each the primitive form is a quadrangular prism: but Mr. Haüy has ascertained, that this prism is rectangular in the diopside, while the faces of that of the pyroxene are inclined to each other in angles of about 92° and 88°. Besides, the primitive form of the diopside is subdivisible in the direction of the two diagonals of its bases, while that of the pyroxene is capable of being subdivided only parallel to the greater diagonal.

rectangle, the other in its bases, which are rhombs inclined to the sides of the prism. From this circumstance Mr. Haüy has suggested the name of diopside (double face.)

II. *Indeterminate varieties of form.*

Indeterminate varieties.

3. Compressed and laminiform diopside*. While Mr. Haüy was examining the crystalline forms of the new species, Mr. Tondi, a mineralogist of distinguished merit belonging to the Museum of Natural History, looking over the collection Mr. Bonvoisin had sent, which was accompanied by a systematic catalogue, observed the flattened variety among some specimens of a different species. This variety, which belongs to the mussite, afforded Mr. Haüy the mechanical division, by which the species is characterized. Compressed & laminiform.

4. Cylindroid diopside: in prisms full of grooves or striæ. Cylindroid.

5. Compact diopside. If we examine attentively the crystals of mussite, we see them prolonged in an uninterrupted series into a compact mass, which serves as their gangue, is of the same colour, though often not so dark, and cannot fail to be perceived to be the same substance, though in a less perfect state of crystallization, as Mr. Bonvoisin conjectured. Compact.

The colours of the diopside are green, greenish gray, greenish white, and yellowish white. It is sometimes translucid, sometimes opake. Colours.

The crystals of mussite are small, elongated, and commonly opake. Several are twisted, and exhibit the primitive form very undecidedly. The crystals of alalite are in general larger, translucid, and of a greenish white. Crystals.

The mussite has been found in the commune of la Balme-de-Mussa, in the department of the Po, toward the north of the valley of Lans, in the interstices of a vein one or two yards thick, that traverses, at the height of four or five yards, a rock called the Black Rock, which is twelve or fifteen yards high. The crystals have sometimes a translucid granular carbonate of lime for their gangue. Mussite where found.

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* This variety of form answers to what Werner calls *strahliger*, radiated.

Alalite where found.

The alalite has been discovered in a vein in the mountain of Ciarmetta, situate beyond that of Testa-Ciarva, at the Alp of la Mussa, near the village of Ala. It is commonly accompanied with green or pale yellow primitive garnets, and emarginated garnets of a hyacinth red, which have nothing in common with the topaz. It was these last that Mr. Bonvoisin designated by the name of topazolites, because he thought them of a pleasing colour, which he compared with the yellow of the topaz.

The pyrophysalite a topaz

I shall avail myself of this opportunity to announce, that Mr. Haüy has found the substance called pyrophysalite by Messrs. Hisinger and Berzelius, an analysis of which was given by these gentlemen in the Annales de Chimie for May, 1806*, to be a variety of the aluminous fluate of silex (topaz), of a greenish white colour, and nearly opake.

IV.

Letter from Sir THOMAS CLARGES, *Bart. of Sutton upon Derwent, to* W. ANNESLEY, *Esq., on the Subject of Life-boats.*

DEAR SIR, *Portsmouth, Nov.* 29, 1808.

Lukin's patent boat.

I Promised to give you an account of Life-boats. In the first place there is one by Lukin, late a coach-maker in Long-Acre, who has published a treatise on it. It is a row-boat, and on the sides has something similar to mine, a hollow case made air-tight, running the whole length of the boat. A bird must have wings on each side to support it in the air; and thus we technically give the name of wings to those parts on the sides of a boat, that tend to give it buoyancy. On the outside of the airtight cases in Lukin's boat is a layer or belt of cork: so that his wings are formed of airtight cases, to buoy up the boat in the water like bladders, and of cork. To prevent the boat from upsetting, it is furnished with a cast iron keel.

Buoyed up by cork, and air-tight cases.

Balanced by an iron keel.

Objections to it.

The objections to Mr. Lukin's boat are these. The airtight cases are not in compartments, or chambers, like mine; and therefore, if forced through by a rock, or striking againſt any projecting part of the wreck, the whole length would fill with water. Besides, they do not open at top like mine, nor, if they did, are they large enough to afford cabin room. Cork, if used in sufficient quantity to be serviceable, occupies considerable space, and likewise affords no room. The iron keel, though safe for sailing, is a very great incumbrance in rowing, particularly against a high wind and heavy sea, so that the boat could not move very fast. My boat, being large and wide, could never be upset, unless the men did it purposely; yet would answer briskly and steadily to the oars.

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* See our Journal, vol. XIX, p. 33.

Shields, or Greathead's boat.

I now come to the Shields boat, improperly called Greathead's. This is sharp at both ends, like the funny-boats about Westminster bridge, to row with either end foremost. The wings are of cork, of which there are two layers, or belts, on each side, one without the gunwale, the other within. The history of the Shield's boat in brief is this. Some gentlemen of Shields, seeing the frequent occurrence of the dreadful accident of shipwreck, offered a premium of two guineas to the person, who should produce the best model of a life-boat; and a committee was appointed, to examine and decide on the models.

Wouldhave's invention.

A painter of Newcastle, of the name of Wouldhave, made a model of copper, sharp and high at both ends, and belted with cork. This, being tried in a tub of water, would not sink when full; and when upset, it righted again.

Greathead's.

Mr. Greathead, a shipbuilder of Shields, gave in a model of a boat he had seen in America, in shape much resembling a butcher's tray. This had a singular property for a life boat, it soon upset, and they could not get it back again, but it whold keep its bottom uppermost.

The committee however rejected Wouldhave's, because copper would not swim; and gave the prize to Greathead, because his model was made of wood. Two gentlemen of the committee went into a brickmaker's, moulded a model in clay, nearly the same shape as Wouldhave's, and floated with cork like his, and gave orders to Greathead to build a boat from this model. Greathead suggested a curved keel instead of a straight one, which was adopted. Thus his claim to invention was building boats of wood, and making them like rocking boats.

Objections to this boat.

The objections to this boat are glaring. The rowing against a heavy sea and wind with such elevated ends is like rowing against the wind with sails set. The idea of the gentlemen was to keep out the water. But what signified the water coming in, if the boat would not sink? Besides, a boat thus formed would be filled amidships by the first wave, in spite of the high ends. Then as the oars lie on a curved gunwale, the poor rowers at each end will be scarcely able to dip their blades in the water; while those in the middle will be constantly catching crabs, as the sailors call it, and getting severe blows on the stomach. The curved keel too has the great inconvenience of making the boat rest as it were on a point, so as to be liable every moment to be turned round, and laid athwart the waves: and if she be not turned round, when a heavy wave strikes against her high bow, from being sharp at both ends she will run backward much faster than the rowers can pull her forward. Now mine is intended only to row forward, and her broad stern hinders her from being driven backward. There is one advantage however in rowing either end foremost; as the boat is liable from her curved keel to be spun round every minute, the men need not concern themselves about getting her round again, but may row on stern foremost, as if nothing had happened. In fact the Shields boat is a farce to a real seaman, and never attempts any thing but moderate seas*. It is indeed much worse constructed for the purpose than any common row-boat, in all respects except the addition of cork; and this even Wouldhave did not invent in the eye of the law, since Lukin had a patent for it years before. Wouldhave's idea of high ends, to make the boat right herself again, if she upset, though not so bad as that of keeping the water out, is like the proverb of penny-wise and pound-foolish: for you cannot well upset a large wide boat in rowing, and such ends may defeat the chief object, that of being able to put out to a wreck against a tremendous wind and sea, which is often to be expected. Another fault of a curved keel is the difficulty it occasions of steering the boat straight, particularly when from the height of the end the steersman is so high above the water.

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* See our Journal, vol. XXI, p. 132: and for a description of a life boat by Mr. Wilson, p. 124.

Description of a new life boat.

To come to my boat, of which a plan is given in plate 2. You will perceive from her size and width, that she is secure from upsetting, without being incumbered below with an iron keel, or above with high ends. She cannot sink, which is the chief thing. Her wings are close; and yet they can be opened to admit many things. The space of the boat is not occupied by belts of cork, or hollow cases that cannot open, like Lukin's: while the wings, from their size, afford cabin room; and, being in compartments, if a rock force through one, the others remaining sound will prevent the water from rising much in that which is damaged. Perhaps Mr. Lukin would say, if these be opened, and any thing put in them, they will cease to be wings, and will not buoy up the boat like a hollow case: but you know one great secret of buoyancy is the resistance at bottom. Thus put an empty basin into water, and try to sink it, and you will find how great its resistance is, though completely open at top: or by putting weights into it you may satisfy yourself, that it will float a considerable burden. In mine the burden can be suited to the occasion; and if not wanted to hold any thing, keep the lids close, and they are airtight buoys. As the wings of my boat are so broad, they form a deck, on which people may lie, particularly if a few iron stanchions be placed along the gunwale and connected by ropes. These stanchions might have a joint at the bottom, allowing them to yield inwardly, but not outwardly, which would prevent any damage if a heavy sea should roll the boat against the wreck.

Her peculiarities. A large cabin,

You must remember a peculiar feature in the boat, the large cabin in the bow for women and children, with ventilators on the valve principle, to admit air upwards, but no water downwards. I need not dwell on the importance of this cabin, in cases of shipwreck, to affrighted and half drowned women and children. It is obviously an indispensable requisite to a life-boat, that it should be incapable of sinking; but mine cannot even fill, or be water-logged. This is effected by very simple means, which do not appear

and after ports to discharge the water.

even to have been thought of before. It must be observed, that the little space in the midships, where the rowers sit, is the only part in which the water, that breaks in, can lodge. But as the bottom of this space is above the level of the water without, there are two portholes at the stern, opening outwardly like valves, at which the water will discharge itself; and when they fall too, the heaviest sea striking the stern, instead of finding its way in, will only serve to close them.

Her good properties.

Lines floated by cork.

Peculiar rudder.

Copper cases not essential.

Having stated the leading features of my boat, namely, that she will not upset, or sink, or be waterlogged; that she affords cabin room; that she is like a man of war's launch, well built for rowing, the oars not on a curve, but nearly in a right line, and low to the water, of which she draws little, a circumstance that renders her a fine pilot boat; I need not dwell on a few secondary points, which however it would be improper not to mention. These are her being provided with small ropes, or lines, fastened to hooks on the gunwale, and each having a piece of cork painted red at the extremity; intended not only for persons who fall overboard, or swim from a wreck, to see and catch hold of; but to tow any for whom there may not be room in the boat: and her having a very powerful rudder, which reaches some inches below the keel, but will haul up level with it, when going in very shallow water, and then let down again. The copper cases, though affording additional security to those who choose to be at the expense, are no more a necessary point of my plan, than coppering her bottom. The woodwork alone, if well executed and properly attended to, may be kept quite airtight. If the assistance of cork were to be called in, it appears to me, that it might be better applied than in the other boats, by filling the cases with cork jackets, to take to a crowded wreck; in going off to which the cases would not be wanted for any other purpose, and the jackets would not be an encumbrance. You must be aware of the importance of the side cabins, or cases, for stowing valuable goods from a richly laden vessel.

Useful to ships on discovery, or any large ships.

A boat of this kind, but somewhat smaller dimensions, would be exceedingly useful to ships on voyages of discovery, and indeed to any large vessels; as it would not only answer for wooding and watering, but is peculiarly adapted for excursions up rivers, or small inlets of the sea, or exploring clusters of islands.

Excellent pleasure boat.

As a pleasure boat she answers extremely well. And with respect to her safety I can say, that I have sailed in her from Brighton round the Cornish coast to Conway in North Wales, without any accident, though we experienced some very dreadful weather on the voyage.

Description of this boat.

The following is a description of the boat, as built by Mr. Christopher Towill, of Teignmouth, see Plate 2.

Her length is 30 feet, her breadth 10, her depth 3 feet 6 inches. The space between her timbers is fitted up with pine wood; this is done with a view to prevent the water lodging there. The pine wood is well caulked and paid.

She is buoyed up by 8 metal cases, 4 on each side; these are watertight, and independent of each other. They will serve to buoy up six tons; but I find that all the buoyant parts of the boat, taken collectively, will buoy up ten tons. The cases are securely decked over, and boarded at the sides with pine. There is a scuttle to each case to put goods in; the edges are lined with baize; and over each scuttle, in the case, is one of wood, of a larger size, the margin of which is lined in the same manner to exclude the water. Between the cases are Norwegian balks, bolted to the bottom, fastened to each other by iron clamps, and decked over. The depth of her keel is about 9 inches below the garboard-streak, the dead rising is 4 inches. Her keel is narrow at the under part, and wide above, for the purpose of giving the timber a good bed, which will support the bolts, in case a necessity should arise to encounter sandbanks.

In sailing over a bar, or in places where the water is shallow, the rudder will, with ease, draw up even with the keel, and when in deep water, it will let down, instantly, and with equal facility, a foot below it; in consequence of which advantage, the boat is found to steer remarkably well.

The forecastle of the boat forms a cabin 10 feet wide, 6 feet long, and 4 feet deep, into which women, children, and disabled persons may be put. It is amply supplied with air, by means of two copper ventilators. It is furnished besides with two grapnels, very proper to be thrown on board a wreck, to ride by. The grapnel ropes will assist the sufferers to remove, and escape from the wreck to the boat. She is likewise equipped with masts and sails, and is as manageable with them as any boat of her dimensions can possibly be.

In a tempest, however, she must be dismasted, and rowed by fourteen men, with oars 16 feet long, double banked. The men are all fastened to the thwarts by ropes, and cannot be washed from their seats.

Explanation of the plate.

Explanation of the plate.

Fig. 1. A. Four copper ventilators.

B. The forecastle skuttle.

C, C, C, C. Four wood scuttles on each side,

D, D. The deck that covers the copper cases, wherein seven tons of goods may be put, or other articles.

E, Hooks for the life lines,

F. Seven life-lines on each side, the ends in the water, floated with cork, by which men may hold that are washed from the wreck before they can be taken into the boat.

G. The grating in the bottom of the boat.

H. Three small pump wells.

I. The tholes.

K. The bitts.

L, L. Ropes to fasten the men to the thwarts.

Fig. 2. Section of her midship part,

M. Five Norway baulks.

N, N. The copper cases.

Fig. 3. A perspective view of one of the cases detached.

Fig. 4. The stern.

O. The rudder on a new principle.

P, P. The stern ports to let out the water.

Fig. 5. A side view of the boat, to show her sheer.

Q, Q. The oars.

V.

Fig. 1.

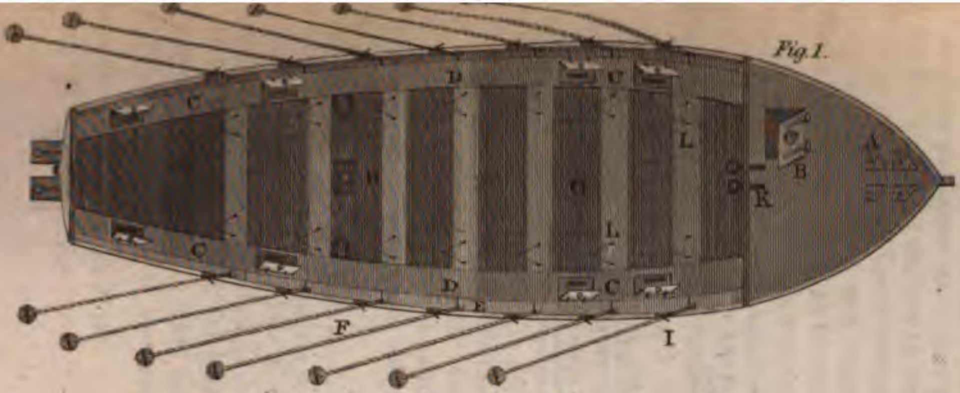


Fig. 2.

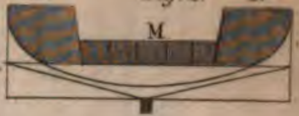


Fig. 3

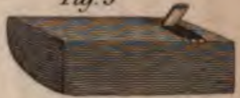


Fig. 5.

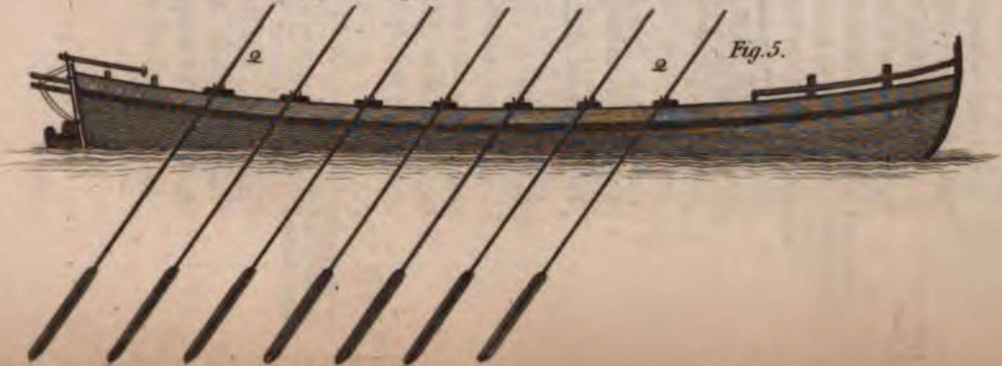
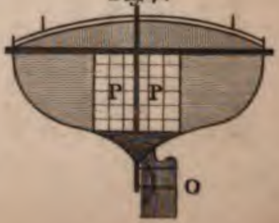


Fig. 4.



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V.

On the Origin and Office of the Alburnum of Trees. In a Letter from T. A. Knight, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. K. B. P. R. S*[*].

My Dear Sir,

The bark not changed into alburnum

IN my last communication I endeavoured to prove, that the bark of trees is not subsequently transmuted into alburnum; and if the statements that I have there given be correct, they are, I conceive, decisive on the point for which I contended: and if the bark be not converted into alburnum, the experiments of Duhamel, and subsequent naturalists, and those of which I have given an account in former memoirs, afford sufficient evidence, that the bark deposits the alburnous matter. If the succulent shoot of a horse chesnut, or other tree, be examined, at successive periods in the spring, it will be seen, that the alburnum is deposited, and its tubes arranged, in ridges beneath the cortical vessels; and the number of these ridges, at the base of each leaf, will be found to correspond accurately with the number of apertures through which the vessels pass from the leaf-stalks into the interior bark, the alburnous matter being apparently deposited (as I have endeavoured to prove in former memoirs) by a fluid which descends from the leaves, and subsequently secretes through the bark[†]. I shall therefore venture to conclude, that it is thus deposited, and shall proceed to enquire into the origin and office of the alburnous tubes.

but deposits the alburnous matter.

Origin and office of the alburnous tubes.

The position and direction of these tubes have induced almost all naturalists to consider them as the passages through which the sap ascends; and at their first formation, when the substance which surrounds them is still soft and succulent, they are always filled with the fluid, which has apparently secreted from the bark. They appear to be formed in the soft cellular mass, which becomes the future alburnum, as receptacles of this fluid, to which they may either afford a passage upwards, or simply retain it as reservoirs, till absorbed, and carried off, by the surrounding cellular substance. The former supposition is, at first view, the most probable; but the latter is much more consistent with the circumstances, that I shall proceed to state.

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[*] Philos. Trans. for 1808, Part II, p. 313.

[†] Phil. Trans. 1807, p. 336.

Ascent of the sap.

Saussure's hypothesis,

and one of Mr. Knight's,

inconsistent with facts.

Many different hypotheses have been offered by naturalists to account for the force, with which the sap ascends in the spring; of these hypotheses two only appear in any degree adequate to the effects produced. Saussure jun. supposes, that the tubes contract as soon as they have received the sap in the root, and that this contraction, commencing in the root, proceeds upwards, impelling the sap before it: and I have suggested, that the expansion and contraction of the compressed cellular, or laminated substance (the tissu cellulaire of Duhamel and Mirbel) which expands and contracts with change of temperature* after the tree has ceased to live, might produce similar effects, by occasioning nearly a similar motion and compression of the tubes, the coats of which are, I believe, universally admitted not to be membranous. But both these hypotheses are inconsistent with the facts, that I have now the pleasure to communicate to you.

Tubes of annual shoots confined to the external annual layer of wood on the same side.

The communication cut off in the stem of some,

and the tubes cut through in others.

Selecting parts of the stems of young trees, from which annual branches had sprung in the preceding year, I ascertained by injecting coloured infusions into the stems, through the annual shoots, that the tubes, which descended from the latter, were, at their bases, confined to that side of the stem from which they spring, and to the external annual layer of wood. Deep incisions were then made into the stems of other trees immediately beneath the bases of similar annual shoots, by which I am quite confident, that all communication through the alburnous tubes, with the stem, was wholly cut off: yet the sap passed into the annual shoots in the succeeding spring, all of which lived, and some grew with considerable vigour. I, at the same time, selected many lateral branches, about three lines in diameter, in a nursery of apple trees, which I could easily secure to the stems of the adjoin-

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* Phil. Trans. 1801, p. 345.

g trees to prevent their being broken. I then made an
cision, more than two lines deep in each, on one side, and
t the distance of six or seven lines another incision, equally
eep, on the opposite side; and as I am quite certain, from
he texture of these branches, that the alburnous tubes
assed straight through them, I am equally certain, that every
lburnous tube was at least once intersected. Yet the sap
assed into these branches, and their buds unfolded in the
cceeding spring, the incisions having been made in the
inter. But I have repeated the same experiment after the
aves have been full grown in the summer, and still the
ranches have continued to live.

The sap still passed, the buds unfolded, and the branches lived.

All naturalists have agreed in stating, that trees perspire
ost in the summer, when their leaves have attained their
ll growth, and of course that much sap must ascend at
is period; yet at this period the tubes of the alburnum
ppear dry, and to contain air only; which induced Grew to
ppose, that the sap rose in the state of vapour; a suppo-
tion by no means admissible. Yet it is, I conceive, evi-
nt, that the sap cannot rise as a liquid through dry
bes, nor in any state through intersected tubes; and there-
re it appears probable, that it does not rise at all through
e tubes of the alburnum, and that these tubes are in-
nded to execute a different office.

When trees perspire most, their alburnous tubes dry.

The sap therefore does not rise through them.

If the sap do not rise through the tubes of the alburnum,
must rise through the cellular substance; yet the passage
any fluid through this has been denied by almost every
turalist, probably because coloured infusions have not
en observed to penetrate it, and because many naturalists
ve considered it as mere compressed medulla. Mirbel,
wever, contends, that the fluid which generates the new
rk exudes from it; and although a fluid, capable of pro-
cing the same effects, exudes from the bark, when de-
hed from the alburnum, I am much disposed to coincide
th him in opinion, having observed a new bark to be ge-
rated on the surface of the cellular substance of pollard
ks, in detached spaces*. And if the sap in sufficient
antity to generate a new bark can pass through the cel-

Consequently it must rise through the cellular substance.

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* Phil. Trans. 1807, p. 107; or our Journal, vol. XIX, p. 245.

lular

Coloured infusions not passing, no proof that sap does not.

lular substance of an oak, it appears possible at lea the whole of the sap may ascend through it. Colou fusions do not, I think, in any degree, pass through t of trees, yet it is evident, that the sap passes readily t it; and therefore should it be proved, that such infus not penetrate the cellular substance of the alburnu evidence which this circumstance would afford would defective.

But coloured infusions will pass through the cellular substance.

Among other experiments which I made to ascerta ther the cellular substance of the alburnum would coloured infusions, I took off branches of two years c the annual shoots and leaves attached to them, in th mer, from trees of different species; and I effectuall the alburnous tubes with a composition formed of c oyster shells and cheese*, and this was covered with ture of bees wax and turpentine, so as effectually to all moisture. A part of the bark was taken off each l in a circle round it, a few lines distant from its low where the tubes had been closed; and each branch w placed in a decoction of logwood, in a vessel deep en cover the decorticated spaces. At the end of twenty or somewhat longer periods, these branches were ex and the coloured infusion was found to have insinuat between the alburnous tubes, in many instances app through the cellular substance. This was most ob the walnut tree, the young wood of which is ver The principal object I had in view in making this ment was, to detect the passages through which I co the sap to pass from the bark into the alburnum†.

The sap secretes through the cellular substance of the alburnum.

From the preceding circumstances, I am disposed fer, that the sap secretes through the cellular subst the alburnum; and through this I conceive that it n cend, when the tubes were intersected in the preced periments, and in those seasons of the year when th nous tubes are empty, though the sap must be risin great rapidity: and I shall endeavour to show, that t

Effectual cement to stop the bleeding of trees.

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[*] I have found this composition, and this only, to be capat stantaneously stopping the effusion of sap from the vine, or oth the bleeding season.

[†] Phil. Trans. 1807, p. 107; or Journal, XIX, p. 245.

sence of the sap in the alburnous tubes, during that part of the year in which trees, when wounded, bleed abundantly, does not afford any decisive evidence of the ascent of the sap through those tubes.

Experiments with the sycamore.

In the last spring, when the buds of the sycamore first began to prepare for unfolding, I found, that the sap abounded in the points at the annual branches; and at the same time it flowed abundantly from incisions made into the alburnum near the root. But when similar incisions were made at the distance of eight or ten feet from the ground, not the least moisture flowed; and the tubes of the alburnum appeared to contain air only. I also observed, that the sap flowed as abundantly from the upper as from the under side of the lower incisions, if not more abundantly, and so it continued to flow to the end of the bleeding season.

The sap must therefore have been, by some means, thrown into the tubes above the incisions, for the quantity discharged from them exceeded more than a hundred times that which the tubes could have contained at the time the incisions were made, even had every tube been filled to the extremity of the most distant branch. And, as it has been shown, that the sap can pass up when all the alburnous tubes are intersected, there appears, I think, sufficient evidence, that it must in this case have been raised by some other agent than those tubes.

The sap ascends through the cellular substance.

Through the cellular substance I therefore venture to conclude that the sap ascends; and it is not, I think, difficult to conceive, that this substance may give the impulse, with which the sap is known to ascend in the spring. I have shown, that the bark more readily transmits the descending sap towards the roots than towards the points of the branches*; and if the cellular substance of the alburnum expand and contract, and be so organized as to permit the sap to escape more readily upwards from one cell to another, than in any other direction, it will be readily expelled to the extremities of the branches: and I have shown, that the statement, so often repeated in the writings of naturalists, of a power in the alburnum to transmit the sap with equal facility in op-

The sap not transmitted with equal facility in opposite directions.

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* Phil. Trans. 1804, p. 5; or Journal, vol. X, p. 292.

posite directions, and as well through inverted cuttings as others, is totally erroneous†.

The albumous tubes reservoirs of sap.

If the sap be raised in the manner I have suggested, much of it will probably accumulate in the alburnum in the spring; because the powers of vegetable life are, at that period, more active than at any other season; and the leaves are not then prepared to throw off any part of it by transpiration. And the cellular substance, being then filled, may discharge a part of its contents into the alburnous tubes, which again become reservoirs, and are filled to a greater or less height, in proportion to the vigour of the tree, and the state of the soil and season: and if the tubes which are thus filled be divided, the sap will flow out of them, and the tree will be said to bleed. But as soon as the leaves are unfolded, and begin to execute their office, the sap will be drawn from its reservoirs, and the tree will cease to bleed, if wounded.

These tubes are analogous to the cavities in bones, increasing strength without weight.

The alburnous tubes appear to answer another purpose in trees, and to be analogous, in some degree, in their effects, to the cavities in the bones of animals; by which any degree of strength, that is necessary, is given with less expenditure of materials, or the encumbrance of unnecessary weight; and the wood of many different species of trees is thus made, at the same time, very light, and very strong, the rigid vegetable fibres being placed at greater distances from each other by the intervention of alburnous tubes, and consequently acting with greater mechanical advantage, than they would if placed immediately in contact with each other.

The air in the alburnous tubes conduces to the production of sugar,

I have shown in a former communication, that the specific gravity of the sap increases during its ascent in the spring, and that saccharine matter is generated, which did not previously exist in the alburnum, or in the sap, as it rose from the root: and I conceive it not to be improbable, that the air contained in the alburnous tubes may be instrumental in the generation of this saccharine matter. For I discovered

as when apples are ground to make cider,

in the last autumn, that much air is absorbed, or at least disappears, during the process of grinding apples for the purpose of making cider, and that during this absorption of

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* Phil. Trans, 1804, p. 5; or Journal, vol. X, p. 292.

air, the juice of acid apples becomes very sweet, and acquires many degrees of increased specific gravity; and a similar absorption of air, with corresponding effects, is well known to take place in the process of malting.

and in malting.

I shall conclude with observing, that in retracting the opinion I formerly entertained respecting the ascent of the sap in the alburnous tubes, I do not mean to retract any opinion that I have given in former communications respecting the subsequent motion of the sap through the central vessels, the leaves, and bark; or the subsequent junction of the descending with the ascending current in the alburnum: every experiment that I have made has, on the contrary, tended to confirm my former conclusions.

Subsequent motion of the sap through the central vessels, leaves, & bark.

I am,
My dear Sir,
Your much obliged obedient servant,
THOMAS ANDREW KNIGHT.

Elton, June 15, 1808.

VI.

Letter on Polygonal Numbers, in Reply to Mr. GOUGH*: by* P. BARLOW, *Esq.*

To the EDITOR of the PHILOSOPHICAL JOURNAL.

SIR,

MR. Gough is perfectly correct in stating, that I was in possession of his reply previous to the date of my letter inserted in your Journal. The reason I did not think proper to publish it was, that it was written in answer to a private letter, which was not exactly the same in form as that published in your number for October, and consequently an answer to the former could not with propriety have been given as a reply to the latter.

Reply to Mr. Gough.

I must now beg leave to be permitted to make a few remarks on the answers of Mr. Gough inserted in your last number;

Reply to Mr. Gough.

number; this gentleman has divided my paper into three objections, which he has pronounced to be *futile*, and I must add without proving them to be so. Mr. Gough will recollect, that in my letter, so far as related to the first four propositions and their corollaries, I did not object to the conclusion, but to the manner of obtaining it; my object was only to show, that an unnecessary number of propositions and corollaries were introduced into the essay, to demonstrate that which needed no demonstration. Mr. Gough does not deny this, except that I have called that a postulate, to which I have given the importance of a theorem, and demonstrated it as such; it is true, as it stands in my letter, it has the appearance of a proposition, but it was unnecessary to give it this form: it might have stood thus;

Definition. Unity is a polygon of every denomination.

Postulate. Let it be granted, that every integer is an aggregate of units, or of polygonals.

Here it is evident that no demonstration is necessary, and had Mr. Gough begun his essay with this postulate, he would at all events have saved himself and readers considerable trouble.

My third, and *only* objection that effects the *truth* of the demonstration, Mr. Gough has evaded, by charging me with putting a false construction upon his words; and now I am under the necessity of retorting the charge. I never said, nor intended to say, that it was necessary to show, that $e = y + t$ can be resolved into $m - f$ polygon in all cases; the question which I proposed was this; "If $e = y + t$ *cannot* be resolved into $m - f$ polygons, how does it appear *from the demonstration*, that $e + f$ can be resolved into m polygons?" This question I again repeat, and unless it can be satisfactorily answered, the theorem will be still without a demonstration, however unwilling Mr. Gough may be to acknowledge it. This gentleman must also be aware, that he cannot be allowed to introduce his examples, which are the moment before derived from the demonstration, to prove the truth of the supposition on which that demonstration is founded.

Mr. Gough appears to have deceived himself by considering only small numbers, in which the polygons, being

also

Reply to Mr. Gough.

also small, are commonly some of them equal to y, m or unity; let him take a larger number, for example 1520, and resolve into squares; now $e = 1520$, $y = 1444$, and $t = 19\,m$; whereas his demonstration does not include numbers higher than those of the form $e = y + 2\,m$.

It is not my intention to pursue this argument any farther, because however curious the theorem may be as relating to the indeterminate analysis, it is perhaps uninteresting to the greater part of your correspondents. If therefore, Mr. Gough undertake to answer this question, I shall leave it for your mathematical readers to decide for themselves, how far it may be considered as satisfactory.

Yours, &c.

Royal Military Academy, P. BARLOW.
Dec. 9, 1808.

VII.

A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced upon them, by the internal Use of solvent Medicines, from Mr. WILLIAM BRANDE *to* EVERARD HOME, *Esq. F. R. S.*

DEAR SIR,

Calculi in the Hunterian Museum and Mr. Homes,

HAVING availed myself of the opportunity you procured for me of making a chemical examination of the calculi contained in the Hunterian Museum, as well as those in your own collection, I herewith send you an account of what I have done.

Should the observations appear to you to throw any new light upon the formation of calculi, I request, that you will do me the honour of laying them before the Royal Society.

very numerous, and in general with

The collection, which I have examined, is not only un-ommonly large, but the greater part of the specimens have istories of the case annexed to them.

their histories annexed.

This circumstance enabled me not only to ascertain the situations in which the calculi were found, but likewise many of the circumstances attendant on their formation.

I have therefore endeavoured to form an arrangement upon these principles, with a view to render the subject more clear and perspicuous.

SECT. I. *Of Calculi formed in the Kidnies, and voided without having afterwards undergone any Change in the urinary Passages.*

Calculi of the kidneys.

These have the following properties:

They are of a brownish yellow colour, sometimes of a grayish hue, which seems to arise from a small portion of dry mucus adhering to their surface.

They are entirely soluble in a solution of pure potash, and during their solution they seldom emit an odour of ammonia.

When heated to dryness, with nitric acid, the residuum is of a fine and permanent colour.

Contain animal matter.

When exposed to the action of the blow-pipe, they blacken and emit a strong odour of burning animal matter, very different from that of pure uric acid. This arises from a variable proportion of animal matter which they contain, and which occasions the loss in the analysis of these calculi. Its relative quantity is liable to much variation, as may be seen from the following statements.

A calculus from the kidney, weighing seven grains, was dissolved in a solution of pure potash. A quantity of muriatic acid (rather more than sufficient for the saturation of the potash) was added, and the precipitate of uric acid thus obtained weighed when dry 4·5 grains. No other substance, except animal matter, which was evident on attempting to obtain the muriate of potash, could be detected, consequently the composition of this calculus was as follows:

	Grs.
Uric acid	4·5
Animal matter	2·5
	7·0

This is the largest proportion of animal matter which I have met with.

Sometimes in very small quantity.

A small calculus from the kidney, weighing 3·7 grains, afforded by a like treatment 3·5 grains of uric acid, so that it was nearly a pure specimen of that substance.

The largest calculus of this kind which I have examined weighed seventeen grains; much larger ones have been found, but there is no evidence of their not having remained in the urinary passages for some considerable time. Thus Dr. Heberden mentions one weighing twenty-eight grains*.

Sand.

It often happens that the ingredients are not united together so as to form a calculus, but are voided in the state of a fine powder, commonly termed sand. This consists either of uric acid, or of the ammoniaco-magnesian phosphate, alone, or with the phosphate of lime.

Phosphates found in this, but not in the calculi.

I am induced to believe, that the last mentioned substances, although the production of the kidnies, and held in solution, are never met with in a separate state, till the urine has been at rest, and therefore calculi from the kidnies are never composed of the phosphates.

Oxalate of lime.

In a few instances, calculi from the kidnies composed of oxalate of lime are voided; but this is a very rare occurrence. Of three preserved in the Hunterian collection, two are extremely small and hard, having an appearance of being made up of several smaller calculi, of a dark brown colour. The third is of the size of a small pea, its surface smooth, and of a gray colour, not very hard.

SECT. II. *Of Calculi which have been retained in the Kidney.*

Calculi detained in the kidney.

When one or more of the calculi described in the preceding section are detained in the infundibula or pelvis of the kidney, it frequently happens, that they increase in that situation to a considerable size.

This increase is of two kinds.

1. Where there is a great disposition to the formation of uric acid, the calculus consists wholly of that substance and animal matter, so as frequently to form a complete cast of the pelvis of the kidney.

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* Comment on the Hist. and Cure of Diseases, 3d. edit. p. 88.

Phosphates in these.

2. Where there is less disposition to form uric acid, the external laminæ are composed of the ammoniaco-magnesian phosphate, and phosphate of lime.

In one instance, a small uric calculus has been deposited in the kidney in such a situation, that its upper surface was exposed to a continual stream of urine, upon which beautiful crystals of the triple phosphate had been deposited. It would therefore seem, that, under common circumstances, a stream of urine passing over a calculus of uric acid has a tendency to deposit the phosphate upon it.

SECT. III. *Of Calculi of the urinary Bladder.*

Calculi of the bladder.

Calculi met with in the bladder are of four kinds.

1. Those formed upon nuclei of uric acid from the kidney.

2. Those formed upon nuclei of oxalate of lime from the kidney.

3. Those formed upon sand, or animal mucus, deposited in the bladder.

4. Those formed upon extraneous bodies introduced into the bladder.

They were arranged under the following divisions.

Calculi of uric acid chiefly.

1. Calculi, which, from their external appearance, consist chiefly of uric acid.

These calculi vary in colour from a deep reddish brown, to a pale yellowish brown.

They are either entirely soluble in a solution of pure potash, or nearly so.

During their solution they frequently emit the odour of ammonia.

When acetic acid is added to their alkaline solution, a precipitate possessing the properties of uric acid is obtained.

Calculi of phosphates chiefly.

2. Calculi composed chiefly of the ammoniaco-magnesian phosphate, or of phosphate of lime, or of mixtures of the two.

These calculi are externally of a whiter appearance than the former.

Some perfectly white, others gray, occasionally exhibiting small prismatic crystals upon their surface; others again soft and friable, a good deal resembling chalk. They are farther characterised by their solubility in dilute muriatic acid.

3. Calculi containing oxalate of lime; commonly called mulberry calculi.

With oxalate of lime, or mulberry calculi.

These are distinguished by the difficulty with which they dissolve in dilute acids, by their hardness, and by leaving pure lime, when exposed to the action of the blow-pipe.

In the examination of these calculi, I was struck with the small number of those strictly belonging to the first division, having been led, from the account of Fourcroy and Vauquelin*, and the experiments of Dr. Pearson†, to believe that calculi, composed of pure uric acid, were by no means unfrequent.

The greater number of the calculi examined by the former chemists are stated to be completely soluble in the fixed alkaline lies; and of three hundred examined by Dr. Pearson, a large proportion is said to consist of uric acid.

The following is a statement of the composition of the different calculi found in the bladder which I have examined.

Composition of different calculi.

16 were composed of uric acid.
45 ——— uric acid with a small relative proportion of the phosphates.
66 ——— the phosphates, with a relatively small proportion of uric acid.
12 ——— of the phosphates entirely.
5 ——— of uric acid, with the phosphates and nuclei of oxalate of lime.
6 ——— chiefly of oxalate of lime.
——
150

To injure these calculi as little as possible, they were carefully cut through with a fine saw, and a portion of the whole cut surface removed by a file; in this way all the different ingredients of the calculi were obtained.

Sawn in two and a portion filed off.

In the experiments upon uric calculi from the bladder, I found, in most instances, a far more considerable loss in attempting

More loss from those of the

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* Annales de Chimie, xxxii, 218.

† Philos. Trans. 1798. p. 37.

bladder than those of the kidney.

tempting to obtain their pure uric acid, than in the kidney calculi, which led me to suppose, that they contained urea; and that the presence of this substance, with some of the salts of urine, and with small portions of the ammoniaco-magnesian phosphate, was the cause of the occasional evolution of ammonia when treated with the fixed alkalis, and of their easy solubility in those substances.

One supposed to consist of urate of ammonia digested in water.

To determine this point, a small calculus, weighing twenty-five grains, and of the species commonly supposed to consist of urate of ammonia*, was digested for two hours with water in a very moderate heat. The water which had assumed a pale yellow colour was filtered off, and fresh water added to the residuum three successive times, when it appeared, that every thing soluble in that fluid was separated. The insoluble part of the calculus, being now carefully dried and weighed, was found to have lost 5·5 grains.

Solution evaporated.

The aqueous solution was evaporated by a gentle heat, nearly to dryness, and a substance was obtained having all the properties of urea, in combination with a small portion of muriate of ammonia, and of the ammoniaco-magnesian phosphate.

Another digested in alcohol.

Sixty grains of another calculus of a considerable size, supposed from a superficial analysis, to consist of nearly pure urate of ammonia, were digested at a low temperature in one ounce of alcohol. In an hour the alcohol was decanted off, and fresh portions were added successively, as long as it appeared to act upon the calculus, which, after having been carefully dried in a temperature below 212°, weighed 54·8 grains, so that 5·2 grains had been taken up by the alcohol.

Solution evaporated.

On evaporating the alcoholic solutions, a substance was obtained having all the properties of urea, with a small portion of saline matter, probably muriate of ammonia, as by the addition of potash a slight ammoniacal odour was perceptible; its quantity however was too minute for accurate examination.

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* Fourcroy observes, that urate of ammonia is easily detected by its rapid solubility in the fixed alkalis, and the odour of ammonia, which is perceived during its solution.—Vide Thomson's Syst. of Chem. vol. 1, p. 691.

The

Treated with acetic acid.

The remaining portion of the calculus, weighing 54·8 rains, was treated with small portions of acetic acid, by hich 6 grains of the ammoniaco-magnesian phosphate were btained.

Residuum dissolved in potash.

The part of the calculus remaining after this treatment, eighing 48·8 grains, was perfectly soluble in a solution of ure potash; it emitted no ammoniacal odour when acted pon by the alkali, and possessed the properties of pure uric cid.

Component parts.

The following therefore is the composition of this calculus.

	Grains.
Urea, and muriate of ammonia	5·2
Ammoniaco-magnesian phosphate	6·
Uric acid	48·8
	60·

No urate of ammonia in alcohol.

From these and many similar experiments upon other alculi, hitherto generally supposed to consist of urate of mmonia, I am induced to believe, that the evolution of mmonia depends in all instances upon the decomposition f the ammoniacal salts contained in the calculus, more esecially of the ammoniaco-magnesian phosphate, and that o substance, which can be called *urate of ammonia*, exists n calculi.

Mulberry calculi.

The mulberry calculus (oxalate of lime) I have but rarely et with. In those preserved in the Hunterian Collection here is a large relative proportion of phosphate of lime, and f uric acid. The purest of them afforded

	Grs.
Oxalate of lime	65·
Uric acid	16·
Phosphate of lime	15·
Loss in animal matter	4·
	100·

Very large calculi seldom homogeneous.

When calculi of the urinary bladder increase to a very large ize, they are generally composed of two or even three of he above mentioned varieties, the ammoniaco-magnesian phosphate being situate externally, and in the greatest abundance.

One of 23 oz. 26 grs.

The largest calculus, which I have seen, weighed, when recently removed from the bladder, twenty-three ounces and twenty-six grains. It consisted of a large mulberry or oxalate of lime calculus, the nucleus of which was uric acid, surrounded by a considerable quantity of the ammoniaco-magnesian phosphate in a very pure state.

One of 15½ oz.

Another very large calculus, weighing fifteen ounces and a half, consisted of a nucleus of uric acid, enveloped in the ammoniaco-magnesian phosphate, not however pure, but intersected by several laminæ of uric acid.

Four distinct substances rare.

Four distinct substances are extremely rare in calculi; I have seen one in which the uric acid, the ammoniaco-magnesian phosphate, the phosphate of lime, and the oxalate of lime, were all in perfectly separate and distinct layers.

Four formed on foreign nuclei.

Four calculi, having the following extraneous substances for their nuclei were examined.

1. A common garden pea.
2. A needle.
3. A hazel nut.
4. A part of a common bougie.

In the two first instances, the calculous depositions were of a pale gray colour, inclining to white; soft and friable in their texture, and entirely soluble in muriatic acid.

The composition of the first was as follows;

	Grs.
Phosphate of lime	65·
Ammoniaco-magnesian phosphate	28·
Loss	7·
	100·

Of the second;

Phosphate of lime	45·
Ammoniaco-magnesian phosphate	38·
Oxalate of lime	12·
Loss	5·
	100·*

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[*] It appears, that in this case there had been an accidental disposition to the formation of oxalate of lime.

The deposition of calculous matter upon the bougie was covered with blood, and in very small quantity, the bougie having been removed by an operation soon after it had passed into the bladder. It appeared to consist chiefly of phosphate of lime.

The incrustation upon the hazel nut was also destitute of uric acid.

SECT. IV. *Of the Calculi of the Urethra:*

Calculi of the urethra.

All those that were examined had escaped from the bladder while very small, and had afterward lodged in the membranous part of the urethra, where they had increased in size, and formed a cavity, in which they were more or less imbedded.

Two of these calculi were broken.

The fragments consisted in one instance of ammoniaco-magnesian phosphate, and phosphate of lime, with a small portion of uric acid: and in the other the fragments were composed entirely of the ammoniaco-magnesian phosphate.

The third calculus was of a very remarkable appearance; its form being that of a perfect sphere, about half an inch in diameter. It was coated with small but very regular crystals of the triple phosphate in its purest state. On account of the singularity of the form and external appearance of this calculus, it was not sawn through; the nucleus, in all probability, is a small kidney calculus, which lodging in the urethra has become coated with triple phosphate.

SECT. V. *Analysis of Calculi from other Animals.*

Calculi of the horse.

1. The horse.

A. *From the kidney.*

From the kidney.

A very large calculus, from the kidney of a horse, was composed of

Phosphate of lime	76
Carbonate of lime	22
	98.

B. *From the bladder.*

From the bladder.

This calculus was also of a large size; its weight, when perfectly dry, nine ounces and a half; its external surface very

SECT. VI. *General Inferences.*

General conclusions.

It appears from the preceding observations, that calculi formed in the kidnies, and immediately voided, are almost always composed of uric acid; and that the phosphates are very frequent ingredients in calculi of the bladder, more especially in those, which, from their situation, have been exposed to a continual current of urine: they also uniformly are deposited upon extraneous substances introduced into the bladder, but appear never to form small kidney calculi.

Fit of the gravel.

In what is commonly called a fit of the gravel, a small uric calculus is formed in the kidney, and passes along the ureter into the bladder.

Formation of the stone in the bladder.

It is found from observation, that for some time after a stone has passed from the kidney, the urine is generally unusually loaded with uric acid, and deposites that substance upon the nucleus now in the bladder. When this period, which is longer or shorter in different individuals, has elapsed, the subsequent addition to the calculus consists principally of the phosphates.

Where the disposition therefore to form uric acid in the kidnies is very great and permanent, the calculus found in the bladder is principally composed of uric acid; but where this disposition is weak and of short duration, the nucleus only is uric acid, and the bulk of the stone is composed of the phosphates.

Where the increased secretion of uric acid returns at intervals, the calculus is composed of alternate layers of uric acid and the phosphates.

Other small calculi being formed in the kidney, they make their way into the bladder, and afford fresh nuclei; so that several calculi are sometimes found in the same bladder, and their composition is usually nearly the same.

In other cases it happens, that a constant increased secretion of uric acid is going on from the kidnies, only in small quantity, which will be more uniformly mixed with the phosphates deposited in the bladder, and where the uric acid predominates, the species of calculus, denominated improperly *urate of ammonia*, will be produced.

Formation of

We are entirely ignorant of the cause of the formation of

oxalate of lime, or mulberry calculus. I have fre- the mulberry calculus.
ntly looked for oxalate of lime in the urine of calculous
ents, but have never been able to detect it; and as it
not exist in healthy urine, it must be regarded as a
bid secretion. Its mode of formation seems to resemble
of uric acid, since small kidney calculi, composed of
ate of lime, have in a few instances been voided; and
hese cases, as far as my own inquiries go, the persons
been much less liable to a return of the complaint,
where uric calculi have been voided.

some rare instances we meet with calculi of the blad-
which are destitute of uric acid, and of oxalate of lime,
nucleus being composed of a little loosely agglutinated
noniaco-magnesian phosphate, and the whole calculus
isting of that substance, with variable portions of phos-
te of lime: in two cases I have met with calculi of this
l, composed of the triple phosphate only: they seem to
ntirely formed in the bladder.

aving taken this short view of the formation of calculi, Action of solvents.
all now inquire into the action of solvents, employed
er with a view of effecting their solution, or of prevent-
their formation and increase.

olvents are of two kinds.

Alkaline. 2. Acid.

the exhibition of these, the practitioner is usually
led by the chemical composition of the calculous mat-
oided by urine.

he different kinds of gravel, voided by persons labour- Gravel of two kinds.
under calculous complaints, may be classed in two di-
ns.

Uric acid, either in a pure state, or with a very small
ortion of the phosphates.

The phosphates, either pure, or with a small propor-
of uric acid.

he first species, which generally appears in the form of Uric acid.
ute crystalline grains, of a reddish brown colour, or of
npalpable brown powder, is either entirely soluble in
alkaline solutions, not emitting an ammoniacal odour,
hich case it consists of pure uric acid: or it does emit
nmoniacal odour, and is not entirely soluble, in which

case it contains the triple phosphate of ammonia and magnesia.

Effect of alkalis.

When this substance is observed in the urine, the alkalis are recommended. They are exhibited either in a pure state, or as carbonates, and in each instance the uric sediment generally diminishes rapidly, and during the continued use of alkaline medicines, occasionally disappears altogether.

It however frequently happens, that the matter voided is not diminished in quantity by the use of alkalis, but that its form and composition are altered, and that it assumes the appearance of a gray powder, and is composed of uric acid with variable portions of the ammoniaco-magnesian phosphate.

Prevent the increase of a calculus.

From these facts therefore it cannot be doubted, that the internal exhibition of alkalis often prevents the formation of uric acid, and hence must likewise prevent the increase of a calculus in the bladder, as far at least as uric acid is concerned; but it has also been supposed, that the alkalis are capable of acting upon the stone itself, and even of effecting its complete solution. It is true, that, if we immerse a calculus composed of uric acid in a dilute solution of caustic alkali, it will be slowly acted upon, and after some time entirely dissolved. If however we attend to what would take place in the body, we shall find the circumstances very different.

Caustic alkali dissolves a calculus out of the bladder.

Alkaline carbonates do not.

Alkalis therefore cannot act on the stone in the bladder.

That alkaline carbonates and subcarbonates exert no action upon uric acid, I consider to be completely established, both by the experiments of several eminent chemists, and those I have myself made upon the subject; and as there is at all times a quantity of uncombined acid in the urine, it follows, that, although the alkali may arrive at the kidnies in its pure state, it will there unite with the uncombined acid, and be rendered incapable of exerting any action upon the calculus in the bladder. Beside phosphoric acid, the urine always contains a quantity of uncombined carbonic acid; this is proved by placing a quantity of recently voided urine under the receiver of an air pump; during the exhaustion, a large quantity of carbonic acid gas makes its escape: and when urine is distilled at very low temperatures, carbonic acid gas is given off: and also, when lime water is poured into urine, a precipitate appears, consisting of phosphate and carbonate of lime.

Limewater more objectionable.

Lime water, on account of the insoluble compound which lime forms with carbonic and phosphoric acids, is even more objectionable as a solvent, than the alkalis.

How far palliative.

It may however be said, that, if these means prevent the increase of a calculus, material relief is afforded to the patient. How far the exhibition of alkaline remedies can be recommended upon these grounds will appear, when the circumstances, which attend the formation of the second species of calculous sediment or deposition in the urine, are considered.

Ammoniaco-magnesian phosphate.

The ammoniaco-magnesian phosphate appears under two forms: it is either voided in a solid state, or in solution. In the former case it bears a good deal of resemblance to a white sand, and is frequently mixed with variable proportions of phosphate of lime. In the latter it makes its appearance after the urine has remained undisturbed for some hours in an open vessel, generally in the form of a fine pellicle, or of crystalline laminæ, which when collected and dried bear some resemblance to boracic acid.

Its putting on this form is accounted for, from its being held in solution in the first instance by carbonic acid, and as this flies off, the triple salt makes its appearance. If a portion of the urine be preserved in a phial closely stopped, the carbonic acid cannot escape, and consequently no phosphate is observed to separate. There is also a quantity of phosphoric acid present, which keeps another portion of the ammoniaco-magnesian phosphate, and also some lime (in the state of superphosphate of lime) in solution.

Injurious effects of alkalis.

It is therefore obvious, that, whenever the urine is deprived of a portion of the acid which is natural to it, the deposition of the triple phosphate, and phosphate of lime, more readily takes place: this is effected by the exhibition of the alkalis.

It may therefore be asserted, that, although alkaline medicines often tend to diminish the quantity of the uric acid, and thus to prevent the addition of that substance in its pure state to a calculus in the bladder; they favour the deposition of the phosphates.

They reach the bladder.

It cannot be doubted, that the alkalis reach the bladder, since in cases where large doses of subcarbonate of potash have been exhibited, I have seen evident traces of it in the urine.

Use of acids.

Where the phosphates only are voided, it has been proposed to dissolve the calculus by the exhibition of acids, and more especially the muriatic acid.

During the use of the muriatic acid, the phosphates are either diminished or disappear altogether; and even sometimes the urine acquires an additional acidity; a solution of that part of the calculus, which consists of the phosphates, might therefore be expected; but even then the nucleus of uric acid would remain, and thus a great deal of time would be lost without any permanent advantage.

I have also occasionally remarked, that during the use of acids, the uric acid reappears, and even seems to be augmented in quantity.

Injection of solvents.

Attempts have been made at different times to effect the solution of calculi, by the injection of solvents into the bladder. This subject has been more lately revived by Fourcroy and Vauquelin, who, in their paper on the composition of calculi, lay down rules for its practice. Independent, however, of the impossibility of ascertaining the composition of the calculus with sufficient accuracy, it is obvious, that were the composition of the surface of the calculus known, the frequent introduction of an instrument into the bladder, and the long continuance of the process which would be necessary, even where the calculi are small, are insurmountable objections; and whenever this mode of treatment has been adopted, it has speedily been relinquished, as it always aggravates the sufferings of the patient.

Real use of alkalis.

It has been shown, that, in the majority of cases, the nuclei of calculi originate in the kidnies, and that of these nuclei by far the greater number consist of uric acid; the good effects therefore, so frequently observed during the use of an alkali, arise, not from any actual solution of calculous matter, but from the power which it possesses of diminishing the secretion of uric acid, and thus preventing the enlargement of the calculus, so that, while of a very small form, it may be voided by the urethra.

VIII.

Some Observations on Mr. BRANDE'S *Paper on Calculi. By* EVERARD HOME, *Esq. F. R. S*[*].

THAT calculi in the human bladder are not dissolved by the internal use of alkaline medicines, is an opinion which I have long entertained, but the grounds of failure, so clearly pointed out by Mr. W. Brande, were not known to me: I only knew from experience, that, to whatever extent the medicines are given, no such effect takes place. The circumstance of the exterior laminæ of calculi extracted from patients, who had persevered in a course of alkaline preparations, having been found softer than the parts towards the centre, has always been considered as a proof of the action of the medicines upon the calculus, and led to the belief, that where the stone was small, it might be wholly dissolved. This, however, Mr. W. Brande has now proved to be a deception, and that the soft part is not a portion of the original calculus, but a newly formed substance, in which the uric acid is not deposited in crystals, but mechanically mixed with the phosphates and the animal mucus in the urine.

Inefficacy of alkaline medicines.

Ground of the contrary opinion

a deception.

Having met with cases, which confirm Mr. W. Brande's observations, it will be satisfactory to state them, as they may assist in doing away many erroneous notions generally entertained on this subject.

Cases confirm this.

The opinion, that calculi in the human bladder have been entirely dissolved, has received its principal support from instances having occurred, and those by no means few in number, where the symptoms went entirely away while the patients were using alkaline medicines, and never afterward returned. This evidence appears to be very strong, but it will be found from the following cases, that it is not so in reality; since the fallacy has been detected in all the instances in which an opportunity was afforded of examining the

Apparently strong evidence

a mere fallacy.

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[*] Phil. Trans. for 1808, p. 244.

the bladder after death. Two of these I shall particularly notice, because they were published during the patient's life time in proof of the stone having been dissolved.

Case illustrating this.

Both patients were great sufferers from the symptoms of stone for many years; but when they arrived at the age of sixty-eight, or thereabout, the symptoms entirely left them. The one had been taking the saline draught in a state of effervescence, under the direction of the late Dr. Hulme: the cure was attributed to this medicine, and the case was published in proof of its efficacy. When the patient died, I examined the bladder, and found twenty calculi; the largest of the size of a hazel nut, the others smaller. It appeared, that the going off of the symptoms had arisen from the posterior lobe of the prostate gland having become enlarged (a change which it frequently undergoes about that period of life,) and having formed a barrier between the calculi and the orifice of the bladder, so that they no longer irritated that part either in the act of making water, or in the different movements of the body, but lay in the lower posterior part of the bladder without producing any disturbance. Their number prevented the pressure from being great upon any one part of the intestine immediately behind the bladder, and their motion on one another rendered their external surface smooth, and probably prevented their rapid increase.

Another.

The other patient was under a course of Perry's lixivium; and when the symptoms went away, he published the case in proof of the efficacy of that medicine in dissolving the stone. I examined the bladder after death, and found fourteen calculi; the largest of the size of a nutmeg, the others smaller. There was the same enlargement of the posterior lobe of the prostate gland, and the calculi were exactly under the same circumstances as in the former case.

In several cases stones not felt.

In several cases, in which I have examined the body after death, calculi have been found enclosed in cysts, formed between the fasciculi of the muscular coat of the bladder, so as to be entirely excluded from the general cavity, and therefore had not produced any of the common symptoms of stone. I have seen in the same bladder, two, three, and even four such cysts, each containing a calculus of the size of a walnut.

It

Mrs. Stevens.

It is a circumstance deserving notice, that in the case, which gave celebrity to Mrs. Stevens's medicine, and procured her a remuneration from Parliament, the bladder was not examined after death.

Calculi sometimes increase during the use of alkalis.

That calculi in the bladder do sometimes increase, while the patient is using alkaline medicines, is fully proved by the following examples, which also show, that the uric acid and phosphates are formed in different proportions, according to the peculiarities of the constitution.

Instance of this.

A gentleman who suffered from symptoms of stone was sounded, and a stone was found in his bladder. I put him on a course of alkaline medicines, and he voided a small compact calculus, composed of uric acid, and evidently formed in the kidney. He was desired to persist in the use of the medicines, which he did at intervals for four or five years, suffering occasionally in a slight degree, but he did not pass any more calculi. He died at the age of seventy-five. On examining the bladder, its whole cavity, (the capacity of which was equal to a pint measure) was completely filled with soft, light, spongy calculi, three hundred and fifty in number, and of different sizes, from that of a walnut to a small pea. They were composed of a mixture of uric acid in powder, the phosphates, and animal mucus; and differed so much from the calculus voided soon after the patient begun the use of alkalis, that they appear to have been formed after that period in the manner mentioned by Mr. W. Brande.

Another.

A gentleman, who was found to have a stone in his bladder, was persuaded, that it was so small, that it might be dissolved, and with this view he took the fossil alkali, both in its caustic and mild state, for about three months; but at the end of that period the symptoms were increased, and he submitted to have it extracted by an operation. On examining the calculus after it was extracted, the external part, for the thickness of $\frac{1}{10}$ of an inch, was entirely composed of triple phosphate, in a state of perfect spiculated crystals, so as to present a very rough irritating surface to the internal membrane of the bladder, while the inner parts of the calculus were made up of a mixture of uric acid and phosphates, so that the alkali had prevented the formation of uric acid, but the phosphates were deposited more rapidly than before.

Alkalis do not always counteract the formation of uric acid.

A gentleman, in whose urine the uric acid appears in a solid form immediately after it is voided, has the same appearance in the urine, even when nine drachms of soda dissolved in water impregnated with carbonic acid are taken in twenty-four hours; so that in this instance the alkali does not even counteract the formation of uric acid.

IX.

Electro-Chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia. By HUMPHRY DAVY, *Esq. Sec. R. S. M. R. I. A.*

(Concluded from Vol. XXI, p. 383.)

V. *On the production of an Amalgam from Ammonia, and on its Nature and Properties.*

Deoxidation and amalgamation of the compound base of ammonia.

IN the communication from Professor Berzelius and Dr. Pontin, which I have already referred to, a most curious and important experiment on the deoxidation and amalgamation of the compound basis of ammonia is mentioned, which these ingenious gentlemen regard as a strict proof of the idea I had formed of its being an oxide with a binary basis.

Mercury, negatively electrified in the Voltaic circuit, is placed in contact with solution of ammonia. Under this agency it gradually increases in volume, and, when expanded to four or five times its former dimensions, becomes a soft solid.

And that this substance is composed of the deoxigenated compound basis of ammonia and mercury, they think is proved; 1. By the reproduction of quicksilver and ammonia, with the absorption of oxigen, when it is exposed to air; and secondly, by its forming ammonia in water, while hidrogen is evolved, and the quicksilver gradually becomes free.

An operation, in which hidrogen and nitrogen exhibit metallic properties, or in which a metallic substance is apparently composed from its elements, cannot fail to fix the attention of chemists: and the peculiar interest, which it offered in its relations to the general theory of electrochemical science, induced me to examine the circumstances connected with it minutely and extensively.

The process repeated.

In repeating the process of the Swedish chemists, I found, that to form an amalgam from fifty or sixty grains of mercury, in contact with saturated solution of ammonia, required a considerable time, and that this amalgam greatly changed even in the short period required for removing it from the solution.

I was however able, in this mode of operating, to witness all the results they have stated, and I soon found simple and more easy means of producing the effect, and circumstances under which it could be more distinctly analysed.

Ammonia probably in its nascent state would be acted on more readily.

The experiments, which I have detailed in the Bakerian lecture for 1806, proved, that ammonia is disengaged from the ammoniacal salts at the negative surface in the Voltaic circuit; and I concluded, that, under this agency, it may be acted on in what is called the nascent state, when it was reasonable to conclude it would be more readily deoxigenated and combined with quicksilver.

50 grs. of mercury placed in a cavity in muriate of ammonia and electrified.

On this view of the subject, I made a cavity in a piece of muriate of ammonia; into this a globule of mercury, weighing about fifty grains, was introduced. The muriate was slightly moistened, so as to be rendered a conductor, and placed on a plate of platina, which was made positive in the circuit of the large battery. The quicksilver was made negative by means of a platina wire. The action of the quicksilver on the salt was immediate; a strong effervescence with much heat took place. The globule in a few minutes had enlarged to five times its former dimensions, and had the appearance of an amalgam of zinc; and metallic crystallizations shot from it, as a centre, round the body of the salt. They had an arborescent appearance, often became coloured at their points of contact with the muriate; and when the connection was broken, rapidly disappeared,

Amalgam produced.

peared, emitting ammoniacal fumes, and reproducing quicksilver.

Carbonate of ammonia produced a similar amalgam,

When a piece of moistened carbonate of ammonia was used, the appearances were the same, and the amalgam was formed with equal rapidity. In this process of deoxidation, when the battery was in powerful action, a black matter formed in the cavity, which there is every reason to believe was carbonaceous matter from the decomposition of the carbonic acid of the carbonate[*].

and some carbon.

Potassium, sodium, &c. employed to deoxidate ammonia without electricity.

The strong attraction of potassium, sodium, and the metals of the alkaline earths for oxigen, induced me to examine whether their deoxidating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and the result was very satisfactory.

When mercury, united to a small quantity of potassium, sodium, barium, or calcium, was made to act upon moistened muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

The amalgam not pure.

As in these cases, however, a portion of metal used for the deoxidation always remained in union in the compound; in describing the properties of the amalgam from ammonia, I shall speak only of that procured by electrical means.

Its properties.

The amalgam from ammonia, when formed at the temperature of 70° or 80, is a soft solid, of the consistence of butter: at the freezing temperatnre it becomes firmer, and a crystallized mass, in which small facets appear, but having no perfectly defined form[†]. Its specific gravity is below 3, water being one.

When thrown into water it produces a quantity of hidrogen,

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[*] The black matter which separates at the negative surface in the electrical experiments on the decomposition of potash or soda, and which some experimenters have found it difficult to account for, is I find carbonaceous, and dependent upon the presence of carbonic acid in the alkali. [See our Journal, vol. XIX, p. 156, and 307]

[†] From the facet I suspect the form to be cubical. The amalgam of potassium crystallizes in cubes as beautiful, and in some cases as large, as those of bismuth.

drogen, equal to about half its bulk, and in consequence of this action the water becomes a weak solution of ammonia.

When it is confined in a given portion of air, the air enlarges considerably in volume, and the quicksilver reappears. Ammoniacal gas, equal to one and a half or one and three fifths of the volume of the amalgam is found to be produced, and a quantity of oxigen equal to one seventh, or one eighth of the ammonia disappears[†].

When thrown into muriatic acid gas, it instantly becomes coated with muriate of ammonia, and a small quantity of hidrogen is disengaged.

In sulphuric acid it becomes coated with sulphate of ammonia and sulphur.

The metal could not be obtained separate.

I attempted by a variety of modes to preserve this amalgam. I had hoped by submitting it to distillation out of the contact of air, or water, or bodies which could furnish oxigen, to be able to obtain the deoxigenated substance, which had been united to the quicksilver in a pure form; but all the circumstances of the experiment opposed themselves to such a result.

It is well known to persons accustomed to barometrical experiments, that mercury, after being once moistened, retains water with great perseverance, and can only be freed from it by boiling; and in the cases of the decomposition of ammonia, when a soft amalgam had been kept continually moist, both internally and externally for some time, it could not be expected, that all the water adhering to it should be easily removed.

I wiped the amalgam as carefully as possible with bibulous paper; but even in this process a considerable portion of the ammonia was regenerated; I attempted to free it from moisture by passing it through fine linen, but a complete decomposition was effected, and nothing was obtained but pure quicksilver.

The whole quantity of the basis of ammonia combined in sixty grains of quicksilver, as is evident from the statements that have been made, does not exceed $\frac{1}{200}$ part of a grain, and to supply oxigen to this scarcely $\frac{1}{1000}$ part of a grain of water would be required, which is a quantity hardly appreciable, and which merely breathing upon the amalgam would be almost sufficient to communicate.

[†] This experiment confirms the opinions I have stated concerning the quantity of oxigen in ammonia; but as water is present, as will be immediately shown, the data for proportions are not perfectly correct.

The amalgam quickly decomposed.

Hence, when an amalgam, which had been wiped by means of bibulous paper, was introduced into naphtha, it decomposed almost as rapidly as in the air, producing ammonia and hidrogen.

In oils it evolved hidrogen, and generated ammoniacal soap; and when it was introduced into a glass tube, closed by a cork, gas was rapidly formed, and the mercury remained free; and this gas, when examined, was found to consist of from about two thirds to three fourths ammonia, and the remainder hidrogen*.

That more moisture sometimes existed attached to the amalgam, when wiped as dry as possible by bibulous paper, than was sufficient for the effect of decomposition, I soon found by an experiment of distillation.

Distilled.

About a quarter of a cubic inch of an amalgam nearly solid was wiped very dry, and introduced into a small tube: in this tube it was heated till the gaseous matter had expelled the quicksilver; the tube was then closed, and suffered to cool, when moisture, which proved to be a saturated solution of ammonia, had precipitated upon it.

The triple amalgams

I have mentioned, that the amalgams obtained from ammonia by means of the metals of the fixed alkalis or alkaline earths seemed to contain much more ammoniacal basis in combination than those procured by electricity: and when they are combined with the metals of the fixed alkalis or of the earths in any considerable quantities, they are much more permanent.

may be preserved some time.

Triple compounds of this kind, when carefully wiped, scarcely produce any ammonia under naphtha, or oil, and may be preserved for a considerable time in closed glass tubes, a little hidrogen being the only product evolved from them.

Heated over mercury.

I heated a triple amalgam obtained from ammonia by potassium, and which had been wiped by bibulous paper, in a dry plate-glass tube over mercury; a considerable elevation of temperature was required before any gaseous matter was emitted, but the heat was raised till gas was rapidly formed, and the whole of the amalgam expelled from the tube: in cooling, the mercury rose very quickly in it, so that a great part of the gaseous matter had been either mercury, or water, in vapour, or something which the mercury had absorbed in cooling. The small quantity, which was permanent, did not equal one half the volume of the amalgam.

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* In the experiment of the action of the amalgam upon air, the oxigen is probably absorbed by nascent hidrogen, and reproduces water, which is dissolved by the ammonia.

The gas examined.

On the idea that this gas might be a compound of hidrogen and nitrogen in the state of deoxigenation, I mixed a small quantity of oxigen gas with it, but no change of volume took place; I then exposed it to naphtha, when one half of it was absorbed, which, by the effect the naphtha produced upon turmeric, must have been ammonia; the remaining gas analysed was found to consist of the oxigen that had been introduced, and of hidrogen and nitrogen to each other in the proportion of nearly four to one.

Here ammonia apparently produced without oxigen.

At first I was perplexed by this result, which seemed to prove the production of ammonia, independent of the presence of any substance, which could furnish oxigen to it, and to show that its amalgamation was merely owing to its being freed from water, and combined with hidrogen: but a satisfactory solution of the difficulty soon offered itself. Exposing the triple amalgam procured from ammonia by potassium to a concentrated solution of ammonia, I found, that it had very little action upon it, and introducing the amalgam moistened by it into a glass tube, it had nearly the same permanency as the amalgam which had been wiped before it was introduced, a little hidrogen only being evolved; but on heating the tube gaseous matter was rapidly generated, which proved to consist of two thirds ammonia, and one third hidrogen.

The difficulty solved.

In the instance in which the amalgam had been wiped, a small quantity of solution of ammonia, and perhaps of potash, must have adhered to it; and though the amalgam does not act upon this powerfully at common temperatures, yet when the water is raised in vapour, it tends to oxigenate both the basis of ammonia, and potassium, and hence hidrogen is evolved, and volatile alkali produced.

Distilled in the vapour of naphtha.

Nothing condensible produced but mercury,

I distilled an amalgam procured by potassium from ammonia, in a tube filled with the vapour of naphtha, and hermetically sealed, in the same manner as in the experiments for obtaining the metals of the earths, but in this case I procured ammonia, hidrogen, and nitrogen only, and pure mercury; and the residuum was potassium, which acted powerfully on the glass tube.

even when cooled by ice.

In another experiment of the same kind, I kept one part of the tube cool by ice, at the same time the other part was strongly heated, but nothing condensible except mercury was produced, and the elastic products were the same as in the former instance.

Ammonia in the state of gas would not amalgamate.

I endeavoured to procure an amalgam from ammonia, to which no moisture could be supposed to adhere, by heating an amalgam of potassium in ammoniacal gas. The amalgam became covered with a film of potash, but it did not enlarge in its dimensions, and a considerable quantity of nonabsorbable gas, which was found to consist of five parts of hidrogen, and one of nitrogen, was produced. The amalgam after this operation did not emit ammonia by exposure to air, hence it seems probable, that for the deoxigenation of ammonia, and the combination of its basis with mercury, the alkali must be in the nascent state, or at least in that condensed form in which it exists in ammoniacal salts, or solutions.

VI. *Some Considerations of general Theory, connected with the Metallization of the Alkalis and the Earths.*

Properties of the amalgam from ammonia extraordinary.

The more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about $\frac{1}{12000}$ part of its weight of new matter is rendered a solid, yet has its specific gravity diminished from 13·5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

It is scarcely possible to conceive, that a substance, which forms with mercury so perfect an amalgam, should not be metallic netallic in its own nature*; and on this idea to assist the iscussion concerning it, it may be conveniently termed mmonium.

But on what do the metallic properties of ammonium de-end?

Are hidrogen and nitrogen metals?

Are hidrogen and nitrogen both metals in the aeriform ate, at the usual temperatures of the atmosphere, bodies f the same character as zinc and quicksilver would be in e heat of ignition?

or oxides?

Or are these gasses, in their common form, oxides, which ecome metallized by deoxidation?

or simple bodies composing a metal?

Or are they simple bodies, not metallic in their own na-ure, but capable of composing a metal in their deoxigen-ted, and an alkali in their oxigenated state?

These problems, the second of which was stated by Mr. avendish to me, and the last of which belongs to Mr. Ber-elius, offer most important objects of investigation.

Amalgam of potassium treated with hidrogen & nitrogen.

I have made some experiments in relation to them, but yet unsuccessfully. I have heated the amalgam of potas-um in contact with both hidrogen and nitrogen, but with-t attaining their metallization; but this fact cannot be nsidered as decisively for or against any one of these con-ctures.

Modification of the phlogistic theory.

I mentioned in the Bakerian Lecture for 1807, that a modi-cation of a phlogistic chemical theory might be defended n the idea, that the metals and inflammable solids usually lled simple, were compounds of the same matter as that xisting in hidrogen, with peculiar unknown bases; and that e oxides, alkalis, and acids were compounds of the same ses with water; and that the phænomena presented by the

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* The nature of the compounds of sulphur and phosphorus with mer-ry favours this opinion; these inflammable bodies by combination im-ir its metallic properties: cinnabar is a nonconductor, and it would em from Pelletier's experiments, Ann. de Chimie, vol. xiii, p. 125, that e phosphuret of mercury is not metallic in its characters; charcoal is conductor, and in plumbago carbon approaches very near to a metal in characters, so that the metallic nature of steel does not militate ainst the reasoning in the text. The only facts which I am acquainted th, that do militate against it, are the metallic characters of some of sulphurets and phosphurets of the imperfect metals.

metals

tainly most easily explained on the hypothesis of I and the only good arguments in favour of a comm ciple of inflammability flow from some of the nov gies in electrochemical science.

Is not hidrogen the common element of inflammable bodies?

Assuming the existence of hidrogen in the am ammonium, its presence in one metallic compound leads to the suspicion of its combination in others. the electrical powers of the different species of ma are circumstances, which extend the idea to co substances in general. Oxigen is the only body, be supposed to be elementary, attracted by the pos face in the electrical circuit; and all compound bo nature of which is known, that are attracted by thi contain a considerable proportion of oxigen. Hi the only matter attracted by the negative surfa can be considered as acting the opposite part t may not then the different inflammable bodies, su be simple, contain this as a common element?

Alkalis, earths, and metallic oxides, belong to the same class of bodies.

Should future experiments prove the truth of t thesis, still the alkalis, the earths, and the metal will belong to the same class of bodies. From potassium there is a regular order of gradation a physical and chemical properties, and this would extend to ammonium, could it be obtained in form. Platina and gold in specific gravity, oxidability, and other qualities differ more from iron, and tin, than these last do from barium and s osmium forms a volatile and acrid substance by the absorption of oxigen, does the amalgam of ammonia produce the volatile alkali; and if we suppose that ammonia is metallized, by being combined with hidrogen, and freed from water, the same reasoning will likewise apply to the other metals, with this difference, that the adherence of their phlogiston or hidrogen would be exactly in the inverse ratio of their attraction for oxigen. In platina* it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any of the metals without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalis, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

Whatever new lights new discoveries may throw upon this subject, still the facts, that have been advanced, show, that a step nearer at least has been attained towards the true knowledge of the nature of the alkalis and the earths†.

We are at least one step advanced in our knowledge of the earths and alkalis.

Something

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* The common metallic oxides are lighter than their bases, but potash and soda are heavier; this fact may be explained on either theory; the density of a compound will be proportional to the attraction of its parts. Platina, having a weak affinity for oxigen, cannot be supposed to condense it in the same degree as potassium does; or if platina and potassium be both compounds of hidrogen, the hidrogen must be attracted in platina with an energy infinitely greater than in potassium. Sulphuric acid is lighter than sulphur; but phosphoric acid, where there is a stronger affinity, is heavier than phosphorus. The oxide of tin (wood tin) is very little inferior to tin in specific gravity. In this instance the metallic base is comparatively light, and the attraction for oxigen strong; and in a case when the metal is much lighter and the attraction for oxigen stronger, it might be expected a priori, that the oxide would be heavier than the base.

Specific gravities of compounds proportional to the attraction of their parts.

† Since the facts in this paper were communicated to the Royal Society, I have seen an account of some very curious experiments of Messrs. Gay Lussac and Thenard, (in Number 148 of the Moniteur, for 1808, which I have just received,) from one of which they have concluded, "that potassium may be a compound of hidrogen and potash."

Potassium supposed to be a compound of hidrogen.

These gentlemen are said to have heated potassium in ammonia, and found, that the ammonia was absorbed, and that hidrogen gas equal to two thirds of its volume appeared, and that the potassium by this pro-

cess

The inflammable body

Something has been separated from them which
their weight; and whether it be considered as oxige

cess had become of a grayish-green colour. By heating this
green substance considerably, two fifths of the ammonia we
emitted, with a quantity of hidrogen and nitrogen correspondin
fifth more; and by adding water to the mixture, and heati
strongly again, they obtained the remainder of the ammonia, and
but potash was left.

But the supposition gratuitous.

In these complex processes, the phænomena may be as e
plained on the idea of potassium being a simple, as that of its
compound substance; nor when the facts that have been stated
paper, and those about to be stated, are considered, can the view
distinguished chemists, as detailed in the notice referred to, be
mitted.

Potash, as I have found by numerous experiments, has no af
ammonia, for it does not absorb it when heated in it; it is not th
allowing their theory, possible to conceive, that a substance h
attraction for potash should repel from it a substance which is in
combined with it, and which can be separated in no other way.

The phenomena accounted for differently.

A part of the hidrogen evolved in their experiment may be f
by water contained in the ammonia; but it is scarcely possible,
whole of it can be derived from this source, for on such an idea
monia must contain more than half its weight of water. There
ever no evidence, that the whole of the hidrogen may not be f
by the decomposition of the volatile alkali itself. Potassium i
degree of oxigenation may have an affinity for nitrogen, or p
may expel a portion of hidrogen at the moment of its combinat
ammonium; and as the whole of the ammonia cannot be regenerat
out the presence of water, hidrogen and a little oxigen may be f
to the remaining elements of the ammonia from the water, and o
the potassium.

Even before the conclusion was formed, that a metallic sub
decomposed in this experiment, it should have been proved, tha
trogen had not been altered.

Potash cannot form potassium by combination with hidrogen.

That mere potash combined with hidrogen cannot form pota
I think shown by an experiment, which I tried, in consequenc
important fact, lately ascertained by Messrs. Gay Lussac and T
of the deoxidation of potash by iron:

Experiment.

An ounce of potash was kept in ignition for some time i
tube, ground into a gun barrel in which one ounce and a hal
turnings were ignited to whiteness; a communication was op
withdrawing a wire which closed the tube containing the potash,
that alkali and the metal.

As the potash came into contact with the iron, gaseous matte
veloped, which was received in a proper apparatus; and though

water, the inflammable body is less compounded, than the inflammable substance resulting from its combustion.

less compounded than

Other

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it was lost by passing through the potash into the atmosphere, yet nearly half a cubic foot was preserved, which proved to be hidrogen. In the tube was found two products, one in the quantity of a few grains, containing potassium, combined with a small quantity of iron, and which had sublimated in the operation, and the other a fixed white metallic substance, which consisted of an alloy of iron and potassium.

The first of these substances burnt when thrown upon water; and in its other characters resembled pure potassium, except that its specific gravity was greater, its colour less brilliant, and when it tarnished in the atmosphere, it became of a much deeper colour than pure potassium.

No water present to have furnished hidrogen,

Now potash that has been ignited is the purest form known of this alkali; but on Messrs. Gay Lussac's and Thenard's theory, this potash must contain water, not only sufficient to furnish hidrogen to metallize the alkali, but likewise the quantity disengaged: dry potash then, as it is procured in our experiments, must on this theory be a compound, containing a considerable quantity of matter which can furnish hidrogen; and what would be its form or properties if deprived of this matter we are wholly unable to judge, which brings this question to the general question discussed in the text.

and none in the potash reproduced.

Potassium I find may be produced readily from dry ignited potash in electrical experiments; and the result of the combustion of potassium in oxigen gas is an alkali, so dry that it produces violent heat, and ebullition when water is added to it.

In Messrs. Gay Lussac's and Thenard's experiment on the action of potassium on ammonia, the hidrogen disengaged in the first process, and that existing in the ammonia disengaged in the second process, exactly equals the whole quantity contained in the ammonia. But there is no proof of any hidrogen being disengaged from the potassium, for the ammonia lost is not generated nor potash formed, but by the addition of a substance, consisting of oxigen and hidrogen; and as the three bodies concerned in this experiment are potassium, ammonia, and water, the result ought to be potash, ammonia, and a quantity of hidrogen, equal to that evolved by the mere action of water on potassium, which is said to be the case.

Chemical properties of potassium very different from that of a compound of potash and hidrogen.

Even if there were no other proofs, the chemical properties of potassium are so wholly unlike those that might be expected from a compound of potash and hidrogen, that they are almost sufficient to decide the question. Potassium acts upon water with much more energy than potash, and produces much more heat in it; and yet if a compound of hidrogen, the affinity of potash for water must be diminished by its affinity for hidrogen, to say nothing of the quantity of heat, which ought (on the common theory of capcaity for heat) to be carried off by this light inflammable gas.

the uninflammable result of its combustion.

The number of chemical elements might be still reduced.

Other hypotheses might be formed upon the new electrochemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained. Certain electrical states always coincide with certain chemical states of bodies. Thus acids are uniformly negative, alkalis positive, and inflammable substances highly positive; and as I have found, acid matters when positively electrified, and alkaline matters when negatively electrified, seem to lose all their peculiar properties and powers of combination. In those instances the chemical qualities are shown to depend upon the electrical powers; and it is not impossible, that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms*.

I venture

Potassium burns in carbonic acid, and precipitates charcoal from it; whereas hidrogen electrized with carbonic acid converts it into gaseous oxide of carbon.

Potash has a very slight attraction for phosphorus; but potassium has a very strong affinity for it, so as to separate it from hidrogen, and according to Messrs. Gay Lussac and Thenard, with the phenomena of inflammation. Potash has no affinity for arsenic, yet from the experiments of these gentlemen it appears that potassium separates arsenic from arseniated hidrogen; and hidrogen, which is supposed by them to exist in both compounds, can have no affinity for hidrogen, nor can hidrogen in one form be supposed capable of separating arsenic from hidrogen in another form.

Could not the experiment of Messrs. Gay Lussac and Thenard be explained, except on the supposition of the hidrogen being derived from the potassium, it would be a distinct fact in favour of the revival of the theory of phlogiston. It would not prove, however, that potassium is composed of hidrogen and potash, but that it is composed of hidrogen and an unknown basis; and that potash is this basis united to water.

* Phil. Trans. 1807, Part I, p. 23, or our Journal, vol. XIX, p. 59.

Difficulties both in the phlogistic and antiphlogistic hypotheses.

The amalgam obtained from ammonia offers difficulties to both the phlogistic and antiphlogistic hypotheses. If we assume the phlogistic hypothesis, then we must assume, that nitrogen, by combining with one fourth of its weight of hidrogen can form an alkali, and by combining with one twelfth more can become metallic. If we reason on the antiphlogistic hypothesis, we must assert, that, though nitrogen has a weaker affinity for oxigen than hidrogen, yet a compound of hidrogen and nitrogen is capable of decomposing water.

The first assumption is however by far the most contradictory to the order of common chemical facts; the last, though it cannot be wholly

removed

Theory of chemistry not yet matured.

I venture to hint at these notions: but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtile powers of matter are but just beginning to be considered; and all general views concerning them must as yet rest upon feeble and imperfect foundations.

Whatever be the fate of the speculative part of the inquiry, the facts however will, I hope, admit of many applications, and explain some phænomena in nature.

Phenomena of volcanoes.

The metals of the Earth cannot exist at the surface of the globe; but it is very possible, that they may form a part of the interior; and such an assumption would offer a theory for the phænomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat*, and would probably lead to a general hypothesis in geology.

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removed, is yet lessened by analogies. Thus alloys in general, and inflammable compounds, are more oxidable than the simple substances that compose them. Sulphuret of iron at common temperatures decomposes water with facility; whereas sulphur under the same circumstances has no action on water, and iron a very small one. The compound of phosphorus and hidrogen is more inflammable than either of its constituents.

Theory of the dependence of chemical forms on electrical powers.

Should a new theory of the dependence of the chemical forms of matter upon electrical powers be established, the facts belonging to ammonium would admit of a more easy solution. Ammonium might be supposed to be a simple body, which, by combining with different quantities of water, and in different states of electricity, formed nitrogen, ammonia, atmospherical air, nitrous oxide, nitrous gas, and nitric acid.

Water, on this idea, must be supposed a constituent part of all the different gasses; but its electricities in oxigen and hidrogen would probably be the very reverse of what they have been supposed by Mr. Ritter, and some ingenious English inquirers.

Water positively electrified would be hidrogen, water negatively electrified, oxigen; and as in the physical experiments of temperature, ice, added to certain quantities of steam, by an equilibrium of heat produces water, so in the chemical experiment of the generation of water the positive and negative electricity of oxigen and hidrogen in certain proportions would annihilate each other, and water alone be the result. At all events ammonium, whether simple or compound, must be considered as owing its attraction for oxigen to its highly positive electrical state, which is shown by its powerful determination to the negative surface in the Voltaic circuit.

* Let it be assumed, the metals of the earths and alkalis, in alloy with

Meteors.

The luminous appearance of those meteors connected with the fall of stones is one of the extraordinary circumstances of these wonderful phænomena. This effect may be accounted for, by supposing, that the substances, which fall, come into our atmosphere in a metallic state; and that the earths they principally consist of are a result of combustion; but this idea has not the slightest connexion with their origin or cause.

X.

On the supposed universal Distribution of Fossil Coal, in Reply to Mr. B. Cook, *Vol. XXI, page* 292; *and on the Nature and Situations of the extraneous Fossil (Belemnite) analysed by Mr.* J. Acton, *at page* 305, *under the Denomination of a "Crystal" called a Thunder-pick. In a Letter from Mr.* John Farey.

To Mr. NICHOLSON.

SIR,

Assertion, that coal may be found in almost all parts of this country,

IT is sincerely to be regretted, when practical and highly useful papers, like that of your correspondent Mr. B. Cook on the advantage of gas lights, in your number for Dec., contain any assertions or speculations, which, being foreign to the profession or pursuits of the writer, are liable to mix errour with so much of practical and useful truth. I was led to these reflections by the following remarks of your correspondent at page 292. "This country produces a vast "quantity of coal, in almost every part where it is properly "sought for;" if gas lights were generally introduced, it might raise the price of coals, "but it certainly would be a "stimulus to men of landed property to seek for it, where

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common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water must produce the effect of subterranean fire, and a product of earthy and stony matter analogous to lavas.

"to the present it has been supposed a stranger: it would "therefore, if the demand was so much greater, be found, "I am sure, in greater quantities than at present, as miners "would be induced to seek it every where."

a mistake of serious moment.

A greater or more common mistake is not often committed, than this which Mr. Cook has fallen into, in supposing, that coals might be any where met with, if sought for; an errour which has occasioned the useless expenditure of hundreds and sometimes of thousands of pounds, in numerous instances, as some in the vicinity of Boxhill in Sussex can testify, on recent experience. A district passing from Somersetshire, through Gloucestershire, Warwickshire, Leicestershire, Nottinghamshire, Derbyshire, Yorkshire, and Durham; and some local districts to the westward and northward of this line, contain numerous and valuable seams or strata of coal; but to the eastward or southward of this line, *no coal ever has or probably ever will be found*, at practicable mining depths, owing to these south-eastern districts being universally covered by great thickness of *upper* strata to any which contain *coal**, and which upper strata seem to cover many of the intervening spaces in the north-western districts of Britain; while in others of these spaces, the coals and their accompanying strata seem wanting, and *lower* strata, from beneath the coal measures, lie exposed.

Only coal districts.

The thunder-pick not a crystal.

The substance called a thunder-pick, the analysis of which is given by Mr. J. Acton at page 305, is not "a crystal," (as Dr. Woodward supposed) but the exuvia of an animal now unknown, called a *belemnite;* which extraneous fossils are frequently found among alluvial matter, on the surface of ploughed lands, mixed with the ruins of the stratum from which they have been dislodged. A stratum in the clay under the Woburn sand produces belemnites in great numbers throughout its whole course, so does a stratum in the great Bath freestone range of hills (see Walcot's Petrifactions found near Bath, page 34), another in the chalk strata, and perhaps others in the British series of strata.

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* *Bituminated wood* lodged in white clay, such as occurs at Borry Tracey in Devonshire, has often been confounded by sanguine speculators with fossil coal, to the cruel disappointment of themselves and others.

I am, Sir,

Your humble servant,

JOHN FAREY.

Upper Crown Street, Westminster.

XI.

Account of a British Vegetable Product, that may be Substituted for Coffee. In a Letter from Mr. WILLIAM SKRIMSHIRE, *Jun.*

To Mr. NICHOLSON.

SIR,

British substitute for coffee.

IN the first week of October last, I discovered a vegetable product of British growth, which by particular management may prove an excellent substitute for foreign coffee, and immediately made it a subject of communication to the Scientific Society in this place. But as I cannot learn, that the substance in question has ever been applied to a similar purpose by any other person, I conceive the circumstance of sufficient importance, to claim the public attention, and should you coincide with me in this opinion, I shall be happy to have it form an article in your valuable miscellany.

Yours, &c.

W. SKRIMSHIRE, JUN.

British Coffee.

Common yellow water-flag.

The iris pseudacorus, flower de luce, or common yellow water flag, is a plant which grows in great abundance in some marshes, and by the sides of rivers and ditches.

The seeds

The germen, or seed pod, which is here provincially and vulgarly called *old sows,* is well stocked with seeds covered with chesnut coloured husks. These may be readily thrashed from the pods when they are ripe, and, if deposited in a dry place, will keep well for a long time.

This beautiful and ornamental plant is so productive of seeds, that I gathered more than a bushel of them in the space of a few yards, by the side of an old river, in this neighbourhood. Very plentiful.

The seeds of this plant, being roasted in the same manner as coffee is treated, very much resemble it in colour and flavour; but have something more of a saccharine odour approaching to that of the extract of liquorice. However, when carefully prepared, they possess much more of the *aroma* of coffee than is to be found in any of the leguminous and gramineous seeds, that have been treated in the same manner. When roasted as coffee superior to any other substitute.

The government duty upon coffee having been lately taken off, this new product cannot at present be brought to market as a lucrative article of commerce; yet I trust these observations may prove of considerable advantage to those persons, in whose vicinity the iris pseudacorus abounds.

Some persons may perhaps object to the use of these seeds as here recommended, on account of the yellow water flag being a medical plant, possessing so violent a purgative effect upon the human frame, as to render its administration extremely unsafe. I readily grant, that the fresh root is a very drastic cathartic; but I assert, that the other parts of the plant do not possess the same virtues as the root; and that even the root itself, when perfectly dried, is one of the most powerful astringents, that this country produces, and is probably one of the most effectual remedies of this class, that we can employ to remove a diarrhœa, or too great a laxity of the bowels. Besides, I can speak positively from my own experience, that the coffee from the seeds of the yellow water flag is very wholesome and nutritious, in the proportion of half an ounce, or an ounce, to a pint of boiling water. Not cathartic. Even the root, when dried, an astringent. Used by the author.

And as far as a few experiments enable me to form an opinion, I expect, that this product will be found to possess most of the chemical as well as physical properties of the foreign coffee. Has the chemical properties of coffee.

The phenomena which occur in roasting the seeds are so very similar to what take place in foreign coffee during the same process, that I shall presently relate them.

The seeds described.

The seeds of the iris pseudacorus when ripe, fresh gathered, and freed from the husks, are of a dirty brown colour, semitransparent, and tough like horn. They have, if I may be allowed the expression, a leguminous taste. Their form is various, some are circular and thin, others wedge shaped, while others again are conical, resembling minute bulbous roots. They are between three and four lines in breadth, never more than four, and they are seldom more than two lines in thickness, but generally much thinner.

Beside the arillus, which merely covers the crown of the seed, it is closely enveloped by a very thin brown epidermis, which firmly adheres to the rugous surface of the seed, giving it the appearance of very fine shagreen. When this covering is removed, the seed itself is of a yellowish colour. Under the microscope this epidermis appears to consist of a congeries of papillæ distilling an oil from the surface of the seed underneath *.

Effects of roasting.

When the seeds are exposed to heat upon an iron plate, in order to roast them, they at first crackle and are covered with minute blisters, they change to a reddish brown colour, and are rendered opake; they next become dark brown, and almost black, by the carbonization of the epidermis; they now sweat, or appear oily, emit a dense smoke, and acquire the *aroma* of coffee. If they be taken from the fire at this stage of the process, and wrapped in unsized paper, it absorbs the oil, and different parts of it thereby become transparent.

In this state the epidermis, though carbonized, does not easily separate from the seed, but adheres to its oleaginous surface, giving it a very dirty appearance. But if rubbed in a cotton or woollen cloth, or tossed to and fro in a bag but partly filled with them, they may be freed from this carbonaceous matter, and will thus receive a polish that will enable them to bear handling without staining the fingers.

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* The arillus, the epidermis, and even very thin cuttings of the seed itself are exceeding beautiful microscopic objects.

Over-roasting.

If the process of roasting be continued longer, the smoke creases in quantity and density, acquiring a very pene- ating empyreumatic odour, the seeds become black from rbonization, and the aroma is entirely destroyed.

Precautions to be observed.

The two great inconveniences in performing this opera- on with the greatest success are, 1st, the shape of the eds, which occasions an inequality in the roasting, unless ey be continually stirred during the process: 2dly, the ugh consistence of the seeds, which renders it necessary conduct the operation very slowly, for if the fire be too olent, the oil will be burnt and communicate a nauseous avour to the coffee; and if the seeds be not sufficiently ied and hardened by the continuance of the heat, they ill remain too tough for the mill to be able to grind them.

In short, the whole art of roasting them consists in being ble to continue the heat long enough to render the seeds a dark brown colour, perfectly opake, and sufficiently rittle to be readily ground by the mill, without allowing it be so fierce as to carbonize the oil, which exudes from em.

These precautions requisite with coffee.

These very directions are as precisely applicable, and as ecessary to be attended to, in roasting foreign coffee, as in reparing the seeds of the iris pseudacorus.

I have preserved the *aroma* of this British coffee in the reatest perfection, by roasting the seeds in the husks; and ould a method be contrived for separating the roasted seeds om the carbonized husks, which may easily be done, I have o doubt but this would be by far the best method of con- ucting the process.

P. S. I hope the idea of presenting the public with a eneral Index to the Philosophical Journal is not wholly iven up. The work itself is now so considerable a depo- tory of knowledge and multifarious information, that it is ecome a work of daily reference, and the public have a ght to expect a general index. So far from the sale of the ork being diminished by such a measure, I should think it ould be increased by the adoption of a fresh series.

W. S.

XII.

XII.

Account of some ferruginous Rocks serving as Substitutes for Emery. By Mr. BLAVIER, *Mine Engineer*[*].

Corundum generally considered an essential part of emery.

NATURALISTS are agreed in confining the name of emery to a rock containing corundum; but if we were to comprise under this general term every matter capable like the true emery of giving the highest polish and lustre to metals, marbles, granites, and other substances necessary or useful in the arts, we might admit into this class the micaceous iron ore, which occurs in the hollows and on the summit of the granitic table-land between the left bank of the Aveyron and the Viaur.

An iron ore answers the same purposes.

Where found.

This substance is found chiefly, and in the greatest abundance, at the bottom of the mountain of Rodez. Its colour is sometimes gray, at others of a deeper or lighter red, but in either case its fracture is steel-grained. It occurs thus on the banks of the brook of Briane, and at a little distance from Boutonnet, in the commune and arrondissement of Rodez. These ferruginous rocks exist in the hollows in rounded nodules, and in masses, the weight of which sometimes amounts to upward of 5 myriagr. [about 1 cwt.]. May we not suppose, that these nodules are nothing but fragments separated from the veins, that appear of different thicknesses through the quartzose schists deeply tinged with iron, that form the higher hills? This situation is at present well known along the Briane, and it agrees perfectly with the different points, where this mineral has been turned up by the plough. Thus similar blocks are found on the summit of the table-land, particularly in the domain of Puech, and at the side of the monastery, directly south and opposite to the mountain of Rodez. The same ferruginous rock exists in separate and more or less bulky fragments on the back of the hill, that forms the separation between the calcareous band of St. Radegonda and the schistous ground that continues parallel with the left bank of the Aveyron as far as the granitic hill of Levezon. On descending the north-east north-east declivities of this hill, toward the coalpit of Sensac, we meet with this ferruginous ore in the greatest abundance, and always with the reddish colour of wine lees. Farther researches will lead us to the discovery of its bed, the correspondence of which with the mine of Boutonnet will easily be established, since the narrow flat of Saint Radegonda is the only space that separates them.

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[*] Journal des Mines, No. 111, p. 201.

Begun to be be worked.

Be this as it may, I can aver, that all the schistous land abounds with this kind of ore; and the working it has already engaged the attention of one of the proprietors of Boutonnet, who has solicited from government a permission to search for this substance, in order to its being used in the arts as a cheap substitute for emery.

A substitute likewise for English rouge.

A stamping mill with three pestles and a few troughs would be sufficient for the establishment of a manufactory, the produce of which would be the more important, because, after having extracted the coarser emery, which constitutes the principal consumption of workers in marble and some other artists, the last deposit of the washing would yield a substance capable of supplying the place of English rouge for the last polish given to metals and even glass. For this nothing more is necessary, than to repeat the washings, till it is brought to a sufficient degree of fineness.

Might be used for polishing mirrors.

The situation of Rodez is very favourable for such an undertaking; since the manufactories of arms at Tulles and Saint Etienne would occasion a considerable demand for the article; and on the other hand the statuaries of Toulouse and some other neighbouring places would find great advantage in using it. Lastly, it appears, that for polishing looking glasses it might be substituted instead of emery of the first and second quality; and I doubt not but with a little practice and patience the workmen may use it all through the process, when they have learnt to prepare it in a proper manner for grinding and polishing at the same time. The artificial emeries however, which we now know how to compose, prevent our being any longer embarrassed with the difficulty of procuring native emery; and if I recommend the establishment here mentioned, it is particularly on account of the cheapness at which its produce may be obtained, especially by the neighbouring manufacturers.

XIII.

On the Anthophyllite; *by* J. C. Delametherie*.

Anthophyllite found in Norway.

THE anthophyllite described by Schumacher is found at Kongsberg in Norway. It has the appearance of asbestoid, or strahlstein. I have an asbestoid from the Tyrol, that resembles it greatly. Its crystals are prisms, the former of which has not yet been determined. Its colour is a brown green inclining a little to violet. Its specific gravity, according to Karsten, is 3·156†.

Dr. John of Berlin, has analysed this substance, and obtained from a hundred parts,

Its component parts.

Silex	62·66
Alumine	13·33
Oxide of iron	12
Magnesia	4
Lime	3·33
Oxide of magnesia	3·25
	98·57
Loss	1·43
	100.

This analysis evidently approaches near that of the asbestoid; accordingly, I have placed it next the asbestoid in my classification of minerals.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Coal-formation near Durham.

AT the meeting of the society on Saturday the 19th of November, Mr. Mackenzie junior, of Applecross, read a short account of the coal-formation in the vicinity of Durham. From the precise and accurate description communicated by this gentleman, the rocks appear to belong to the oldest coal-formation of Werner. During the course of his observations, he explained what is called the *creep* by miners, and exhibited specimens of the different rocks, and a section of the coal-mine of Kipier, in which both the miners' appellations and the scientific names of the different strata were inserted.

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* Journal de Physique, vol. LXIV, p. 356.

† Haüy gives its spec. grav. 3·292, and suspects it to be merely a variety of the Labrador hornblende. T.

Mineralogy of East Lothian.

At the same meeting, Dr. Ogilby of Dublin read the continuation of his description of East Lothian, under the title of Observations on Veins of the newest Floetz-trap of East Lothian. After some preliminary observations on the general geognostic relations of the rocks of East Lothian, and of the precipitation of feldspar in its different states of fineness, from earthy to glassy feldspar, he proceeded to describe the different veins he had an opportunity of examining in this tract of country. These veins he considered as of three different periods of formation; viz. 1. Veins derived from partial formations subsequent to the floetz-trap, which however are not of frequent occurrence; 2. Veins of the different rocks of the formation penetrating the older beds: and, 3. Those of contemporaneous origin. He next enumerated and described, after the manner of Werner, the following veins,—greenstone, jasper, quartz, heavy-spar, and calc-spar; and concluded with several interesting general remarks.

Great sea-snake.

At this meeting, also, Mr. P. Neill read some observations on the great sea snake of the Northern ocean. He enumerated and read extracts from the different authors, who have mentioned it,—Ramus, Egede, and Pontoppidan. He remarked, that it was placed, by the latter author, between the *mermaid* and the *kraken*, in a chapter which treats on sea monsters; and that, standing in such suspicious company, it had been rejected by naturalists in general as a fabulous creature. He stated however, that, within these few weeks, a vast marine animal, shaped like a snake, and not described in the works of systematic naturalists, had been cast ashore in Orkney. This curious animal, it appears, was stranded in Rothesholm Bay, in the island of Stronsa. Malcolm Laing, Esq., M.P., being in Orkney at the time, communicated the circumstance to his brother Gilbert Laing, Esq., advocate, Edinburgh, on whose property the animal had been stranded. Through this authentic channel Mr. Neill received his information. The creature was dead when it came ashore, and the tail seemed to have been injured and broken by dashing against the rocks. The body measured fifty-five feet in length, and the circumference of the thickest part was equal to the girth of an Orkney poney.

One lately driven ashore at Orkney.

Mr. Malcolm Laing, and are to be deposited in the
of the University of Edinburgh. Mr. Neill
with remarking, that no doubt could be enterta
this was the kind of animal which had served as
type of all the wonderful sea-snakes, whose app
on record; and that although the unfortunate d
of the specimen by the storm may probably ren
possible to form a correct generic character on
principles, yet a place (if it should be in an a
could no longer be refused by the most scrupulo
alists to the serpens marinus magnus of the bisho
gen.

Sea unicorn.

At the meeting of this Society the 10th of Dece
Secretary read a communication from the Rev.
ming of Bressay, describing a narwhal, or sea-u
the sort denominated *le narwal microcephale,*
pède, which had been lately cast ashore alive at
Sound in Zetland. The description was accompa
a correct drawing of the animal, which is to be en

Mineralogy of Fassnet.

At the same meeting, Dr. Ogilby of Dublin re
on the transition greenstone of Fassnet in East
which beside much valuable mineralogical informa
tained a satisfactory answer to the query proposed
ago by Professor Jameson in regard to the goegn
tions of the rocks of this tract of country. Th
tions of the individual rocks, and their general an
lar geognostic relations, were detailed with ability

Officers for 1809.

The following gentlemen have been elected office-bearers of this society for 1809 :—

President. R. Jameson, Esq., Prof. Nat. Hist. Edin.

Vice Presidents. Dr. Wright, Dr. Macknight, Dr. Barclay, and Dr. Thomson.

Of the Council. Gen. Dirom, Col. Fullerton, C. S. Menteith, Esq., Dr. Home, Dr. Yule, James Russell, Esq., C. Anderson, Esq., and C. Stewart Esq.

Treasurer. P. Walker, Esq.

Secretary. P. Neill, Esq.

Carmichael on the effect of oxide of iron on cancer, and the uses of oxide of iron in the blood.

Mr. CARMICHAEL, of Dublin, has in the press a second edition of his Essay on the Effects of Carbonate and other Preparations of Iron upon Cancer, with an Inquiry into the Nature of that Disease. This edition, we understand, is so much enlarged and improved, that it may almost be considered as a new work. Among the additions are a great number of highly interesting cases; a disquisition on the uses of the oxide of iron in the blood; and remarks on such diseases, as depend on its excess or deficiency, or in any way bear a relation to cancer; with an attempt to answer the Queries of the Medical Society established in London for investigating the nature and cure of that complaint.

Scientific institution.

Mr. George Singer, has by some recent arrangements considerably improved the original plan of the Scientific Institution, Prince's-street, Cavendish square; at which in future the public lectures are to be assisted by courses of private instruction, and conversations on the various subjects of philosophical inquiry; which are severally illustrated by an extensive and increasing collection of instruments. The attention of the pupils in the ensuing season will be principally directed to subjects of electrical and chemical research; with particular reference to the developement and explanation of the new experiments. A sketch of the plan of this institution, and a prospectus of the lectures, is preparing; and may be shortly obtained at the lecture room.

London Hospital.

Medical lectures.

Dr. Buxton's Lectures on the theory and practice of Medicine and on materia medica will be commenced about the 20th. January, 1809.

METEOROLOGICAL JOURNAL

For DECEMBER 1808,

Kept by ROBERT BANCKS, Mathematical Instrument Ma
in the STRAND, LONDON.

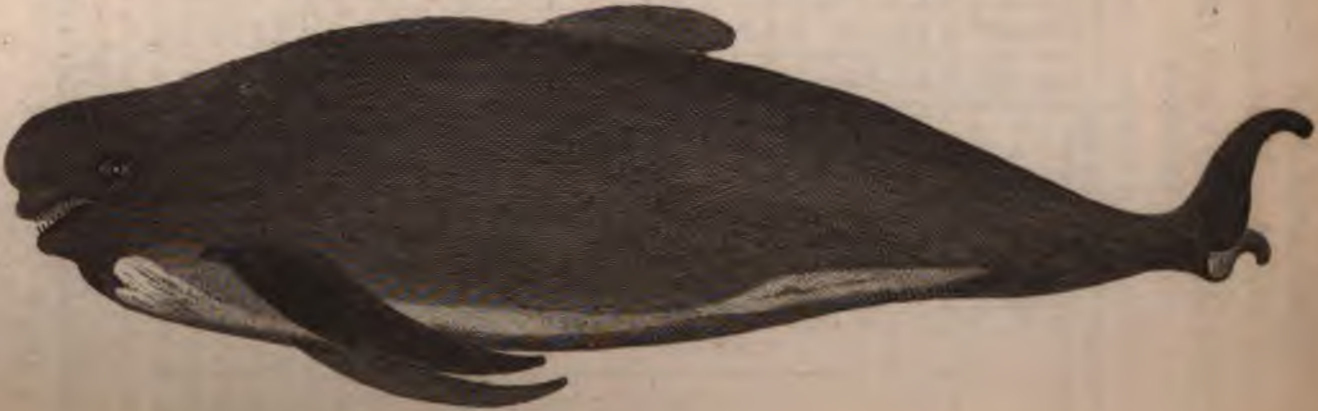
NOV.	THERMOMETER.				BAROMETER,	WEATHER.	
Day of	9 A. M.	9 P. M.	Highest.	Lowest.	9 A. M.	Night.	Day.
26	50	52	54	50	29·98	Cloudy	Rain
27	50	42	54	34	29·50	Fair	Ditto
28	35	36	53	34	29·81	Ditto	Fair
29	36	41	47	40	29·77	Rain	Rain
30	42	41	46	37	29·16	Fair	Fair
DEC.							
1	40	43	47	42	29·36	Ditto*	Ditto
2	41	43	47	41	29·17	Ditto	Ditto
3	42	46	47	42	29·45	Ditto	Ditto
4	41	40	48	36	30·02	Fog	Ditto
5	40	48	49	48	30·27	Mist	Cloudy
6	50	47	52	34	29·97	Fair	Rain
7	35	39	42	32	29·89	Ditto	Fair
8	34	40	41	40	29·95	Ditto	Ditto
9	41	39	44	33	29·86	Fog	Ditto
10	35	40	40	34	30·08	Cloudy	Ditto
11	36	36	42	33	30·25	Fair	Ditto
12	36	41	42	39	30·25	Fog	Cloudy
13	40	35	42	34	30·33	Rain	Ditto
14	34	40	42	33	30·36	Cloudy	Ditto
15	34	33	38	33	30·08	Ditto	Fair
16	33	33	35	30	30·07	Fair	Ditto
17	32	26	34	22	29·82	Snow†	Snow
18	26	30	32	28	29·62	Fair‡	Fair
19	28	29	33	28	29·65	Cloudy	Snow
20	28	32	33	20	29·69	Fair	Ditto
21	24	31	31	28	29·96	Snow	Ditto
22	28	28	32	26	29·26	Ditto	Ditto
23	27	32	32	27	29·38	Ditto	Ditto
24	28	28	33	26	29·47	Cloudy	Cloudy
25	29	30	30	24	29·56	Ditto	Ditto
26	26	31	32	30	29·49	Ditto	Ditto

* Heavy rain in the night.

† Stars brilliant at 6 P. M., snow at 9, high wind all night.

‡ Snow at 5 P. M. Stars brilliant in the evening. At 12 appearance o
change for thaw.

Nicholson's Philos Journal. Vol.XXII. Pl.3. p. 81



Delphinus Melas.
A Nondescript Species of the order Cete,
Ninety two of which were driven ashore in Scapay Bay, a few days
before the great Storm in December 1807.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1809.

ARTICLE I.

Description of a new Species of Whale, Delphinus melas. In a Letter from THOMAS STEWART TRAILL, *M. D.*

To Mr. NICHOLSON.

DEAR SIR,

New species of whale.

A Description and correct drawing of an animal never before figured by any naturalist cannot fail of being acceptable to many of your numerous readers. Ninety-two whales of a new species were stranded in Scapay Bay, in Pomona, one of the Orkneys, a few days previous to the great storm in December, 1806. My friend James Watson, Esq. made the enclosed drawing (see Plate III) on the spot, the day after they were driven on shore.

Differs from the grampus.

This animal very clearly belongs to the genus *delphinus*, of the class *mammalia*. The only hitherto described species of that genus, which it at all resembles, is the *delphinus orca*, or grampus; but it is distinguished from the grampus by the shape of its snout, the shortness of its dorsal fin, the length and narrowness of its pectoral fins, the form and number of its teeth, and the colour of its belly and breast.

Abundant about the Orkney & Shetland islands.

It abounds in the seas around the Orkney and Shetland Isles. In Mr. Neill's interesting Tour through those islands, we are informed, that 310 of this species were forced on shore in Shetland in 1805. From the imperfect account transmitted to him, this gentleman very properly conjectured them to be a new species.

Description.

Description.—The whole body almost is black, smooth, and shining like oiled silk. The back and sides are jetty black; the breast and belly of a somewhat lighter colour. The general length of the full grown ones is about twenty feet. The body is thick. The dorsal fin does not exceed two feet in length, and is rounded at the extremity. The pectoral fins are from six to eight feet in length, narrow, and tapering to their extremities. The head is obtuse; the upper jaw projects several inches over the lower in a blunt process. It has a single spiracle. The full grown have twenty-two subconoid sharp teeth, a little hooked. Among those stranded in Scapay Bay were many young ones, which, as well as the oldest, wanted teeth. The youngest measured about five feet in length, and were still sucklings. The females had two teats, larger than those of a cow, out of which the milk flowed when they were squeezed.

Habits.

These animals are gregarious, generally swimming in considerable numbers. They frequently enter the bays around the Orkney coast in quest of small fish, which seem to be their food. When one of them takes the ground, the rest surround, and endeavour to assist it: from this circumstance several of them are generally taken at once. I have frequently observed an animal, which I conjecture to be of this species, elevating its dorsal fin and a considerable part of its back above the waves, with a slow tumbling motion for many successive times. They are inoffensive, and rather timid. They are chased on shore not unfrequently by a few yawls. They seem generally to follow one as a leader with blind confidence. I once was in a boat when the attempt was made to drive a shoal of them on shore; but when they had approached very near the land, the foremost turned round with a sudden leap, and the whole rushed past us with great velocity, but carefully avoided the boat. They are extremely fat, and yield a considerable quantity of good oil.

This new species may be denominated *delphinus melas*, on the same principle on which Gmelin gives the Beluga the name of delph. leucas. The following may serve as its character. Delphinus Melas.—*Corpore crasso, nigro; pinna dorsali una brevi; pinnis pectoralibus longis, angustis; rostro obtuso; maxillo superiore proclinante; dentibus acutis conoideis, parum incurvatis.*

Name and character.

THOMAS STEWART TRAILL.

Liverpool, Dec. 17, 1808.

II.

Description of an Apparatus for the Analysis of the Compound Inflammable Gasses by slow Combustion; with Experiments on the Gas from Coal, explaining its Application. By William Henry, *M. D., Vice Pres. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester. Communicated by* H. Davy, *Esq., Sec. R. S.*[*]

THE aëriform compounds of hidrogen and carbon, which were already entitled to accurate investigation, as objects of scientific research, have derived an additional claim to the attention of the chemist from their application to an important economical purpose, described in a late communication to the Royal Society[†]. Yet there is, perhaps, no part of chemistry, the investigation of which is beset with greater difficulty, or with more numerous sources of errour; insomuch, that the actual state of the science enables us to attain scarcely more than approximations to the truth, and degrees of probability of greater or less amount. It was the object of the experiments, which are described in the following pages, rather to remove some of the obstacles, which present themselves to a successful inquiry into the nature

Compound inflammable gasses merit accurate examination,

but this liable to many errours,

here attempted to be removed

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[*] Phil. Trans. for 1808, p. 282.

[†] See Mr. Murdoch's paper, p. 124; or Journal, vol. XXI, p. 94.

Best method of proceeding.

nature of these bodies, than to acquire such facts, as may enable the chemical philosopher to decide the controverted questión respecting their composition. Results sufficiently multiplied and precise for this purpose would require a larger appropriation of time, than I have the prospect of being able to bestow; and I can only on the present occasion offer an example of the method, in which it appears to me, that the analysis of this class of substances will be most successfully attempted.

Vegetable substances.

The carbon.

The hidrogen.

The gasses always more or less mixed,

or their elements combined in various proportions.

When a vegetable substançe, composed (as may be assumed to simplify the statement) of oxigen, hidrogen, and carbon, united in the form of a ternary compound, is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities, which constituted the triple combination, is destroyed; and the elements, composing it, are united in a new manner. Those, which are disposed to enter into permanently elastic combinations, escape in the state of gas. The carbon, uniting with oxigen, either composes carbonic acid gas, or, stopping short of that degree of oxigenation, which is essential to change it into an acid, is converted into carbonic oxide. The hidrogen, combining with a portion of carbon, constitutes a binary compound of these two ingredients, forming either what has been called *carburetted hidrogen gas*, or *supercarburetted hidrogen*, better known by the appellation of *olefiant gas*. Toward the close of the process, a portion of simple hidrogen gas is also mingled with the products. Perhaps in no instance is any one of the gasses, which have been enumerated, obtained perfectly pure, by the distillation of a vegetable substance. The aëriform fluids, which are thus generated, are found to be possessed of almost every degree of specific gravity; and to yield, by combustion, extremely different results, according to the temperature at which they have been formed; the stage of the process at which they have been separated; and other modifying circumstances. It becomes an interesting question, whether these gasses, so much diversified in their physical and chemical properties, are mixtures of a few binary compounds, with which chemists are already acquainted; or whether, on the contrary, their elements are capable of uniting in indefinite proportions, and of composing ternary compounds of oxigen, hidrogen, and carbon, or varieties of *oxicarburetted* hidrogen. It would encroach too much on the time of the Royal Society, to enter upon this controversy. And, as neither opinion admits, at present, of demonstrative evidence, I may be permitted, in explaining the following experiments, to assume that theory, which appears to me most probable; viz. that the aëriform products of the distillation of vegetable substances are mixtures of carbonic acid, carbonic oxide, olefiant, carburetted hidrogen, and simple hidrogen gasses; or of two or more of these in various proportions.

Most probably mixtures.

Usual mode of analysis.

The analysis of these compound gasses has hitherto been attempted solely by their rapid combustion with oxigen gas, in the following manner: a mixture of the inflammable gas with oxigen gas in known proportions is admitted into a Volta's eudiometer; inflamed over mercury by the electric spark; and the diminution ascertained. To the remainder caustic potash or lime water is added, by which it sustains a second diminution of bulk; and the amount of this denotes the quantity of carbonic acid formed by the combustion. The quantity of nitrogen gas, in the oxigen employed, as well as in the residue left by potash, being determined by a fit eudiometrical test, it is easy to infer what quantity of oxigen has been absorbed by the detonation. And as it is proved, that oxigen gas sustains no change of bulk by conversion into carbonic acid, we may conclude, that, after deducting from the volume of oxigen gas expended that of the carbonic acid which has been formed, the remaining number shows how much oxigen has been employed in the saturation of hidrogen. If, for example, 100 measures of carburetted hidrogen consume 200 of oxigen gas, and give 100 of carbonic acid; it follows, that the carbonic acid holds in combination 100 measures of the oxigen gas consumed; and that the remaining hundred have been applied to the saturation of hidrogen. In this estimate it is assumed, that the carbon has acquired, by combustion, the whole of the oxigen necessary for its acidification, and that no part of it existed previously in the state of carbonic oxide, a proposition, in many cases, perhaps, very far from being consistent with

One source of errour

may be detected.

with the truth. This, however, admits of being decided by an accurate comparison between the weight of the gasses consumed and that of the products.

Gasses already examined.

For the purpose of obtaining a general approximation to the nature of a combustible gas, it may be sufficient to examine its coincidence with those, the properties of which have been already determined. The following table exhibits the results of the combustion of the few gasses, that appear entitled to be considered as distinct species. They are deduced from the experiments of Mr. Cruikshank and Mr. Dalton.

Kind of Gas.	Sp. Grav. (air = 1000.)	100 measures,		
		Take meas. of oxigen.	Give carbonic acid	Are diminished by firing.
Olefiant - - - - - -	909	300	200	200
Carbonized hidrogen, from stagnant water - -	600	200	100	200
Carbonic oxide - - - -	967	45	90	55
Hidrogen gas - - - -	84	50		154

Inflammability of gasses proportional to the oxigen they consume.

The inflammability of the compound gasses, and their fitness for the purpose of affording light, are directly proportionate to the quantity of oxigen required for their saturation. The olefiant gas, therefore, burns with the greatest brilliancy; carburetted hidrogen gas, though inferior, affords a dense and compact flame; but the carbonic oxide and hidrogen gas are entirely unfit to be employed as the means of artificial illumination.

Results distrusted

because they varied considerably.

In the execution of a series of experiments on the compound combustible gasses, which are described in the 11th volume of Mr. Nicholson's Philosophical Journal, I had reason to be dissatisfied with the above method of effecting their decomposition, and to distrust the results which were obtained. The products of the combustion of the same gas varied considerably in different experiments; and, with respect to some, it was evident, that the full proportion of their carbonaceous ingredient was not oxigenised, in consequence of the precipitation of charcoal in the act of detonation. The quantities, also, that can be submitted in this

Experiments not without danger.

way to experiment, are extremely minute; and the inflammation of highly combustible gasses is attended, as I have more than once experienced, with considerable danger from the bursting of the glass tubes. It was desirable, therefore, to employ a process not liable to these objections; and after many alterations of the apparatus, contrived with this view, I at length fixed upon one, which I shall now proceed to describe.

Apparatus contrived for the analysis described.

The principal parts of the apparatus, are two glass cylinders, or air receivers*, *b b* and *o o*, Pl. IV, of which the larger one is intended to contain oxigen gas, and the smaller one, the inflammable gas submitted to experiment. They are connected by a bent glass tube *s s*, the diameter of which should not be less than $\frac{1}{10}$ of an inch, to the upper extremity of which is cemented an iron burner, *t*, the orifice of which is about $\frac{1}{30}$ of an inch, while to the lower end a socket is fixed, on which may be occasionally screwed the cock *r*. The receiver *o o* is contained in a larger glass jar *n n*, and is closed at the top by a brass cap *p*, and stop cock *q*. The oxigen gas receiver is also closed by a brass cap *e* and cock *f*, the lower orifice of which is tapped internally, for the purpose of receiving a small screw at the end of the copper wire *g*. This wire is in two parts, each of which screws into a movable socket, connecting the two; and, by this contrivance, the wire may be lengthened or shortened at pleasure. To prepare the apparatus for use, the receiver *o o* is partly filled with the combustible gas; and is secured by wedges of cork *v v*, in the jar *n n*, the level of the water in the latter being regulated by opening the cock *x* or *z*. The bent pipe *s s*, with its cock *r*, is screwed upon the top of the receiver, and partly immersed in the water of a pneumatic cistern, *u u*, so that the orifice of the burner may rise a few inches above the surface of the water. The receiver *b b*, detached from the situation in which it is represented in the drawing, is then exhausted by an airpump; and, being filled with oxigen gas, is transferred (its mouth being closed during the act of removal with a piece of leather) to

Method of preparing it for use,

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* I am indebted to Mr. H Creighton, of Soho, not only for a drawing of the apparatus, but for much valuable assistance in the performance of the experiments.

the cistern *a*, and quickly inverted over the burner *t*. By a little practice, this may be done with the admission of very little common air. A transferring vessel is then screwed upon the cock *f*; and a portion of oxigen gas removed for eudiometrical examination. To allow room for the expansion of the oxigen gas, the water is raised by a siphon to a proper height within the receiver *b*, as appears in the drawing.

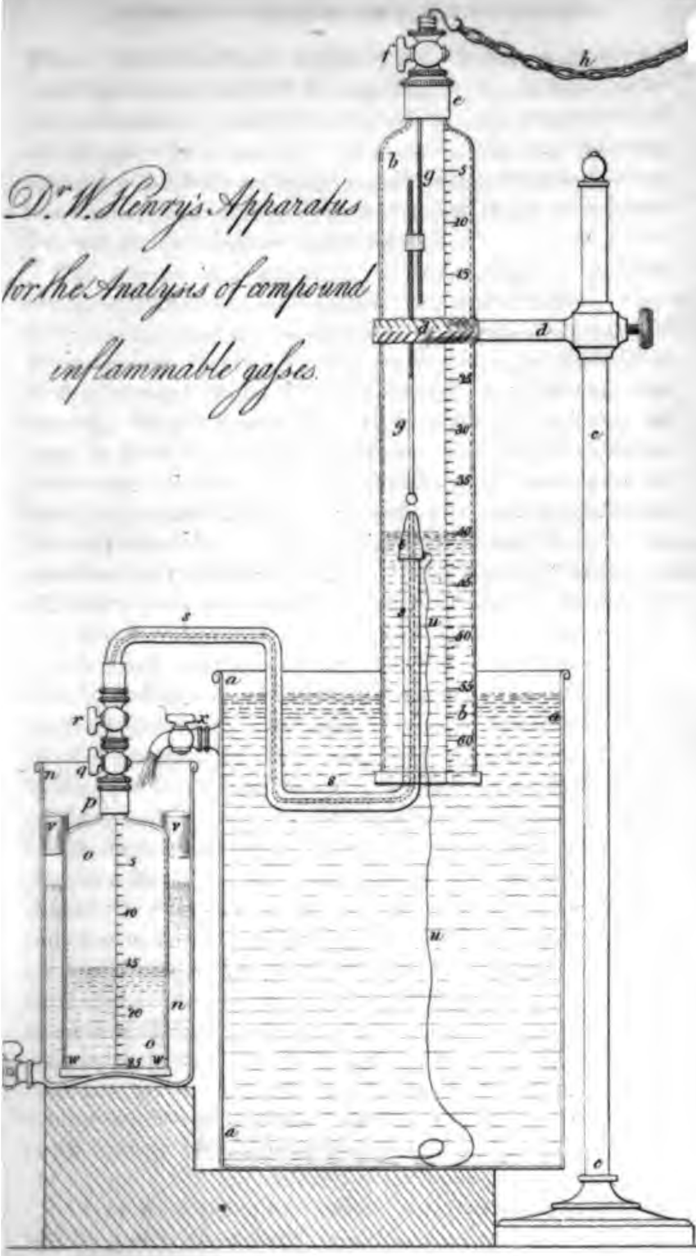
and of conducting the experiment.

The apparatus being thus disposed, the cock *f* is connected by the chain *h* with the prime conductor of an electrical machine; and a rapid succession of sparks is made to pass between the copper ball at the end of the wire *g*, and the orifice of the burner. The cocks *q* and *r* being now opened, the stream of gas is kindled; and in order to prevent the flame from playing upon the wire, the jar *n n* is moved a little nearer to the cistern *a*, which brings the point of the burner into the axis of the receiver. At the same time, by opening the cock *x*, water flows into the jar *n n*, and finds its way into the receiver, through two small holes *w w* drilled near its mouth.

The combustion continues, until either the whole of the inflammable gas is consumed, or till the cocks *q* and *r* are shut. The wedges *v v* are removed; the receiver *o o* unscrewed; and the bent tube removed from its place. It is at this moment, that the cock *r* is useful, by preventing the escape of the gas from the receiver *b* through the tube *s s*. The upper part of the receiver is cooled by the application of a wet sponge. Without waiting, however, till the gas has attained the temperature of the atmosphere, a very small and sensible thermometer is introduced into it; and the height of the mercury is noted, as soon as it becomes stationary. The volume of the residuary gas is then observed, and is reduced by calculation to the bulk, which it would occupy at 60° of Fahrenheit. Either the whole, or an aliquot part of it is removed by a transferring vessel, screwed upon the cock *f*, to a mercurial cistern, where the proportion of carbonic acid is determined by liquid potash. The proportions of oxigen and nitrogen gasses, in the unabsorbed residue, are learned by agitation with sulphuret of lime, observing the precautions which have been stated by de Marti.

Calculation of the results.

Dr. W. Henry's Apparatus for the Analysis of compound inflammable gases.



Marti. The residuary oxigen being deducted from the quantity at the outset of the experiment shows how much oxigen has been expended in the combustion of the inflammable gas. It is scarcely necessary to observe, that the gasses are carefully reduced, at each stage of the operation, to a mean temperature and pressure, (60° of the thermometer, and 30 inches of the barometer)*.

Source of error.

The process of combustion, as thus stated in general terms, appears sufficiently simple. It is often, however, rendered complicated by the imperfect combustion of the inflammable gas, a part of which escapes through the orifice of the burner, either wholly unaltered, or only partially burned. As this portion is not absorbed by sulphuret of lime, it gives a fallacious appearance of an actual addition of nitrogen to the oxigen gas remaining in the receiver *b*. I am unacquainted with any method of entirely obviating this difficulty; but its amount may be diminished by an attention to certain precautions. With this view, the pressure upon the gas, contained in the receiver *o o*, should, on first opening the cocks *q* and *r*, be no more than is sufficient for its gentle expulsion through the tube *s s*. When, however, the stream is once kindled, the larger the flame, and the more active the combustion, within certain limits, the more completely is the gas consumed. It is necessary, also, to stop the combustion, before it is rendered languid by the admixture of carbonic acid with the gas in the receiver *b*, and by the diminished purity of the oxigen gas. If this be not attended to, a large proportion of the inflammable gas, toward the close of the process, makes its escape unaltered into the receiver *b*. In general I have found, that, setting out with oxigen gas of equal purity, the more combustible the inflammable gas submitted to experiment, the more complete is its decomposition by slow combustion. The apparatus, therefore, is better adapted to the analysis of olefiant gas, of carburetted hidrogen gas, or of mixtures of these two, than of carbonic oxide, or any gas of which this oxide forms a large proportion.

Precautions against it.

The more combustible the gas the more complete the decomposition.

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* The rules observed in these calculations are stated in my Epitome of Chemistry, 5th edition, p. 441.

The

Method of ascertaining the residual hidrogen.

The inflammable gas, which has found its way into the receiver *b*, is always present in too minute a quantity to compose, with the residuary oxigen, after the removal of the carbonic acid, a mixture capable of being inflamed by the electric spark. To ascertain its precise quantity, it is necessary to have recourse to another operation. After trying, eudiometrically, the quality of an aliquot part of the gas in the receiver *b*, let a similar aliquot part be deprived of its carbonic acid, and then mixed with a portion of pure hidrogen gas, not exceeding one third or one fourth the estimated bulk of the oxigen which it contains. Detonate the mixture, and observe the amount of the diminution after the explosion; the products of the combustion; and the quantity of oxigen gas consumed. After subtracting, from the total expenditure of oxigen, half the bulk of the added hidrogen gas, the remaining number shows how much oxigen has been absorbed by the combustible gas contained in the residue. By the rule of proportion, it may be determined, how much carbonic acid would have been produced, by the oxigenation of the whole of the combustible gas, and what quantity of oxigen it would have saturated.

Objection. Absorption of part of the carbonic acid by the water.

The most obvious objection to this method of analyzing the compound gasses is, that the real proportion of the products, resulting from their combustion, may perhaps be disguised, in consequence of the absorption of a part of the carbonic acid by the water, over which the experiment is made. By frequent trials, however, I find that this is a source of errour too trivial to be deserving of consideration; and that the proportion of carbonic acid, thus generated, exceeds what is composed by the rapid combustion of the same gas over mercury. When the operator has acquired sufficient dexterity, the interval of time between the completion of the combustion and the admeasurement of the residue is too small, to allow an absorption to any notable amount. It must be observed, also, that the carbonic acid constitutes only a small part of the residue; and is, for this reason, very little acted on by water, conformable to a principle which I have explained in the Philosophical Transactions for 1803, p. 274*. I believe, therefore, that, with an

This trifling.

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* Journal, vol. V, p. 233.

attention to those observances, which are required in all delicate experiments on gasses, and to the changing circumstances of temperature and pressure, this apparatus is fully adequate to the purpose for which it is intended. It will be easy, however, for those, who have the command of a sufficient quantity of mercury, to adapt the apparatus to this fluid. As an exemplification of the method of using it, in the simplest possible case, I shall state the results of the combustion of hidrogen gas.

Results of the combustion of hidrogen gas.

At the outset of the experiment, there was contained in the receiver *o o* a quantity of hidrogen gas, equal, when reduced to a mean temperature and pressure, to 15·8 cubic inches.

Of these, there remained unconsumed 2·5

Hidrogen gas burned.............. 13·3

In the receiver *b* were 49 cubic inches of oxigen gas, consisting of................ 33·5 oxigen, 15·5 nitrogen

At the close of the experiment,
there remained, in *b*, 43·5 *c. i.*
composed of.............. 27·25 16·25

Cubic inches of oxigen gas consumed .... 6·25

But estimating from the first diminution (viz. 49—43·5) only 5·5 cubic inches of oxigen would appear to have been absorbed: and the nitrogen gas, by eudiometrical experiments, would seem to have been increased 0·75 of an inch. As the hidrogen gas, however, had been prepared from zinc and sulphuric acid with extreme caution, and did not contain an appreciable quantity of common air, no such addition of nitrogen could have taken place. The apparent increase, then, may be fairly imputed to the escape of 0·75 of an inch of hidrogen gas, which are to be deducted from the 13·3 cubic inches at the outset of the experiment; and hence the real quantity consumed will be 13·3 — 0·75 = 12·55. The true consumption, also, of oxigen gas was 5·50 + 0·75 = 6·25, or pretty exactly, as it ought to be, half the bulk of the hidrogen, which was actually burned.

Experiments on olefiant gas.

An example of the analysis of a highly combustible species of elastic fluid is furnished by the following experiments on the olefiant gas, obtained from alcohol and sulphuric acid. Of this gas 100 cubic inches, at a mean of the barometer and thermometer, were equal to 30 troy grains; hence its specific gravity was 967.

In the receiver *o o*, were contained of this gas	6·3 cub. in.
Residue ································	2
Gas consumed ························	4·3

In the receiver *b*, were 43·4 inches of oxigen gas. After the combustion, there remained 38·2 cubic inches of mixed gasses, of which 8·6 were carbonic acid. None of the inflammable gas, which passed through the bent tube, had escaped being burned, for the quantity of gas in *b*, not absorbable by sulphuret of lime, so far from having been increased, was found to have sustained a trifling diminution. The oxigen gas, which was consumed, amounted to [illegible] cubic inches. Reducing these results to centesimal proportion, 100 cubic inches of this gas would give 200 of carbonic acid, and absorb 325 of oxigen gas. This experiment agrees with Mr. Dalton's, as to the proportion of carbonic acid from the combustion of olefiant gas, but assigns a larger consumption of oxigen. It may be observed, however, that the specific gravity of the gas, which I employed, exceeded a little the statement of the Dutch chemists, who found its specific gravity to be 909, common air being 1000.

Gasses from vegetable substances.

Having satisfied myself, by repeated experiments, of the accuracy of the results which may be thus obtained, I proceeded to the combustion of the gasses from a variety of vegetable substances, and especially from those which it seemed probable might become economical sources of light. In the present memoir, I shall describe those only, which were made on coal and a few similar substances, reserving the rest for a future communication.

Gas from cannel coal.

Gas from cannel coal.

This was received in two separate portions. Of the first product, 100 cubic inches, corrected to a mean temperature and pressure, weighed 24·28 grains. Hence its specific ity was to that of atmospheric air as 783 to 1000. The nd product was much lighter, 100 inches weighing only grains, and having, therefore, the specific gravity of

The results are comprehended in the following table. carbonic acid, stated to have been generated by the nd combustion, was formed by adding to an aliquot of the residue, after the removal of the carbonic acid, oportion of hidrogen gas; detonating the mixture by electric spark; and proceeding as already directed. The two lines contain the minutes of actual experiments; third and fourth these results reduced to centesimal proion.

c. .	Cubic inches burned.	Oxigen gas consumed.	Carbonic acid generated.	Carb. ac. form'd by 2d com bustion.	Ox. consumed by 2d combustion.	Total oxigen consumed.	Total, carbonic acid formed.
	7·3	16·5	8·3	1·9	0·9	17·4	10·2
	9·8	9·4	4·8	0	0	9·4	4·8
	100	222	113·7	26	12	234	139·7
	100	96	49	0	0	96	49

he early product of the gas from cannel coal, before g washed with lime water or caustic potash, is a mixof several different gasses, viz. carbonic acid, sulphud hidrogen, olefiant, and a fourth, which is either a gas *eneris*, or a mixture of carburetted hidrogen, and carc oxide. To ascertain the proportion of these gasses in mixture, is a problem of some difficulty. Sulphuretted ogen and olefiant gasses experience, it is well known, mediate condensation, when mingled with oximuriatic gas, and in this way they may be separated from carc acid. Again, sulphuretted hidrogen and carbonic are absorbed by liquid potash, which has no action on nt gas. If, therefore, two equal portions of the gas coal be mixed with oximuriatic gas, the one in its restate, the other after being washed with potash, the ensation of the former will be found to exceed that of vashed portion. By the combined use of these agents, ay attain an approximation, at least, to the proportions ich carbonic acid, olefiant, and sulphuretted hidrogen gas

The first product a mixture of gasses.

One sui generis?

Method of ascertaining their proportions.

gas are mingled with the aëriform product of coal. The rule may be stated as follows; to a measured quantity of oximuriatic acid gas, contained in a graduated tube, add twice its bulk of the recent coal gas, and at the expiration of one or two minutes observe the diminution which has taken place. Wash an equal quantity with caustic potash; note the loss; and submit the residue to the action of oximuriatic acid as before. The second diminution, thus effected by oximuriatic gas, divided by 2·2, gives the proportion of olefiant gas. Deduct this absorption from the first, and, dividing the remainder by 1·8, we obtain the quantity of sulphuretted hidrogen. Lastly, to know the quantity of carbonic acid gas, subtract, from the diminution effected by potash, the amount of the sulphuretted hidrogen gas. An example, taken from actual experiment, will best explain the application of this rule.

General rule.

Practical application of it.

One hundred measures of the first product of gas from cannel coal lost, by agitation with liquid potash, 9·7 measures. The remainder, being mingled with one fourth its bulk of oximuriatic acid gas, the mixture lost 10·4 measures. This diminution, 10·4, divided by 2·2, gives 4·9 for the proportion of olefiant gas. But 100 measures of the unwashed gas sustained, by admixture with oximuriatic acid, a diminution of 20 measures. Now, deducting, from this diminution, that occasioned by the condensation of olefiant gas (viz. 20 — 10·4), there remain 9·6, which, divided by 1·8, give 5·3 for the proportion of sulphuretted hidrogen gas. And the diminution by potash (=9·7) — 5·3 gives 4·4 for the proportion of carbonic acid gas. Hence 100 measures of the first product of gas from cannel coal contain,

First gas from cannel coal.

1. Of inflammable gas, not affected by the foregoing agents	85·4
2. Of sulphuretted hidrogen gas	5·3
3. Of olefiant gas	4·9
4. Of carbonic acid gas	4·4
	100

The

Very little common air.

The proportion of common air, in the foregoing specimen of gas, and in all cases when care was taken to exclude it, was too small to deserve being taken into the account, not appearing, by the test of nitrous gas, to exceed 1 per cent.

Composition of gas from different coals, and bitumens.

The following table exhibits the composition of gas from various kinds of coal. In the last column, under the term inflammable gas, is comprehended that portion, which is neither suddenly condensed by oximuriatic acid gas, nor absorbed by potash. A name more descriptive cannot be applied to it, because it varies essentially in different cases, and the proportion of its components is still matter of doubt.

TABLE I.

Kind of coal.	No. of the product	100 measures consisted of			
		Sulph. hid.	Carb. acid.	Olefiant.	Inflam.
Wigan cannel	1	5·3	4·4	4·9	85·4
	2	0.	1·8	0	98·2
Wednesbury, Staffordsh.	1	4·9	3·4	0	91·7
	2	0·	2·8	0	97·2
Newcastle on Tyne....	1	2·9	2·8	2·7	91·6
	2	2·2	1·7	0	96·1
Newcastle, Staffordshire	1	3	2·7	0	94·3
	2	1·4	2	0	96·6
	3	0	1·4	0	98·6
Middleton, near Leeds	1	3	2·	1	94
	2	1·4	1·7	0	96·9
	3	0	2	0	98·
Black Mine, near Manchester	1	3·3	3·2	2·5	91
	2	2	1·1	0	96·9
	3	2	1·	0	97
	4	0·5	1·2	0	98·3
	5	0	1·2	0	98·8
	6	0	0	0	100
Merthyr, Glamorgansh.	1	1	1·7	0	97·3
	2	0	1·7	0	98·3
	3	0	1·6	0	98·4
	4.	0	1·5	0	98·5
	5	0	1·	0	99
	6	0	0	0	100
Native coal tar..........	-	13	6	15	66
Caoutchouc	-	0	4·9	17	78·1

After separating the sulphuretted hidrogen and carbonic acid gasses by agitation with liquid potash, the residue, consisting of the inflammable gas mixed with the proportion of olefiant gas produced along with it, was submitted to combustion. The following table shows the average results of a number of these experiments.

TABLE II.

Results of the combustion of the inflammable gas.

Kind of coal.	No of the product	Weight of 100 cubic inches (Ther. 60° Bar. 30°.)	Spec. grav. (Air 1000.)	100 cubic inches	
				consume oxig. gas	give carbonic acid
		Grs.			
Wigan cannel ••	1	24·28	783	234	139·7
	2	10·4	335	96	49
Wednesbury coal	1	20·9	674	190	97·5
	2	9·8	316	85	46
Newcastle on Tyne••••••••	1	19·3	622	190	100
	2	9·8	316	86	45
Newcastle, Staffordshire ••••	1	19·6	632	195	98
	2	17·7	570	165	80
	3	12·1	390	100	60
Leeds ••••••••	1	20·7	670	190	100
	2	15·1	487	lost by accident.	
	3	9·8	316	85	42
Black Mine, Lancashire ••	1	19·4	627	186	97
	2	15	484	137	65
	3	11·3	364	100	50
	4	10	322	90	47
	5	9·5	307	85	45
	6			80	40
Merthyr ••••••	1	12	387	117	62
	2	9·5	307	90	47
	3	8	261	75	39
	4	5·9	190	60	31
	5	5·8	187	57	26
	6	5·5	177	50	20
Coal tar ••••••••	-	24·2	780	233	150
Caoutchouc ••••••	-	—	—	204	121

General observations.

An attentive examination of the results, contained in both the tables, suggests the following general remarks.

Olefiant gas.

1. The olefiant gas is a very sparing product of the distillation of pit coal. It is found only in the first portions, and even of these it does not compose more than 5 per cent.

Result much influenced by the heat employed.

Its quantity, however, is very much influenced by the temperature employed. This remark, indeed, may be extended to all the aëriform products of coal; insomuch that from equal weights of the same coal it is difficult to obtain by different operations, conducted on a small scale, products which are the same either in quantity or quality. The gas from *Coalbrooke dale* tar, and that from *caoutchouc*, have a larger proportion of olefiant gas, which in them amounts to about one sixth of their bulk.

Sulphuretted hidrogen.

2. Sulphuretted hidrogen gas is, also, most abundantly produced at the early stages of the distillation. Its proportion then varies from 1 to 5 per cent; and towards the close of the process it disappears entirely. It increases the illuminating power of the coal gas; but is by no means a desirable product; since it yields by combustion a gas (the sulphurous acid) which is extremely offensive and irritating to the lungs. By the distillation of coal, more sulphuretted hidrogen is produced, than is discovered among the aëriform products; for a part, uniting with the ammonia, which is generated at the same moment, forms sulphuret of ammonia, a compound which I have found among the condensed products.

Sulphuret of ammonia.

Carbonic acid.

3. Carbonic acid gas, like the two preceding ones, appears only at an early stage of the process, and in small proportion, never amounting to 5 per cent. A portion of this gas, also, unites with ammonia, and hence carbonate of ammonia is found in the condensed fluid.

The product gradually diminishes in gravity and combustibility.

4. The gas from coal undergoes a gradual diminution of specific gravity and combustibility, from the commencement to the close of the process. This is best shown by inspecting the results of the experiments on the *Black-Mine* and *Merthyr* coal gas in Table II, because they were reserved in a greater number of separate portions than usual. The progression would, perhaps, have been more regular, in these as well as in the other instances, if much of the gas had not been allowed to escape, in consequence of the immense quantity which was produced. The specific gravity of the coal gas appears to afford a measure of its fitness for illumination, sufficiently accurate for practical uses; but does not bear an exact correspondence to the

Carbonic acid not always in proportion to the oxigen expended.

chemical properties of the gas, as ascertained by combustion. It may be remarked, also by comparing the two last columns of the second table, that the carbonic acid produced does not always bear the same proportion to the oxigen expended. Thus the first product of gas from cannel coal combines with 234 measures of oxigen gas; and gives 139·7 of carbonic acid. But the gas from coal tar, with only an equal consumption of oxigen, yields 150 measures of carbonic acid.

Coal gas not the same exactly with any other combustible gas.

5. The aëriform product of coal does not precisely answer to the characters of any one of the combustible gasses, with which we are acquainted. The first product, however, of the distillation of common pit coal, after being washed with potash, approaches very nearly in its properties to carburetted hidrogen gas. The gasses, which surpass this in specific gravity, are mixtures of carburetted hidrogen with olefiant gas, and perhaps a small proportion of carbonic oxide. The lighter gasses, in addition to carburetted hidrogen, probably contain a variable proportion of hidrogen gas and a small quantity of carbonic oxide. The extreme levity of some of the products, especially of the gas from *Merthyr* coal, cannot be explained on any other supposition.

Product of the combustion of coal gas.

6. The products of the combustion of a cubic foot of coal gas, of medium quality, viz. of the specific gravity 622, (such as the first products from *Newcastle on Tyne* coal) may be stated as follows:

	Grains.
A cubic foot, at a mean of the barometer and thermometer	333·5
By combustion, it yields 817·3 grains of carbonic acid, the carbon in which may be estimated[*] at	233·7
Grains of hidrogen in a cubic foot of coal gas	99·8

But 99·8 grains of hidrogen are equivalent to the saturation of 554·9 grains of oxigen, with which they form 654·7 grains of water. Hence the oxigen consumed ought from calculation to be 817·3—233·7 = 573·6 + 554·9 = 1128

And the quantity actually consumed appears by experiment to be	1110·3
Errour	17·7

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[*] Assuming the carbon to be 28·6 grains in 100 grains of carbonic acid, as is satisfactorily proved by the experiments of Messrs. Allen and Pepys.

The difference, in this example, between experiment and calculation is not greater, than, in such delicate processes, may always be expected. A part of the deficiency in the oxigen actually consumed may be ascribed, also, to a small portion of the inflammable gas being already in the state of carbonic oxide.

Without repeating the particulars of a similar calculation made on gas of an inferior quality, I shall annex a comparative statement of the specific gravities and composition of the good and inferior gasses.

Difference between the best and inferior gasses.

Source of the Gas.	Weight of a Cubic Foot.	A Cubic Foot consists of		Oxigen Gas consumed by a Cub. Ft.	Gives	
		Carb	Hidr.		Carb, Acid	Water
Newcastle coal	333·5 gr.	233·7	99·8	1110·3	817·3	621
Ditto, last product	169·3	111·5	57·8	560·	400	384·9

The inferior gas, also, probably contains carbonic oxide; and the quantity of oxigen gas, actually consumed, will be found, on calculation, less than it ought to be, if the carbon were not already combined with a portion of oxigen.

The water estimated.

The quantity of water, which was generated by combustion, was not determined experimentally, but is merely estimated. It must be acknowledged, that the decomposition of the inflammable gasses cannot lead to unquestionable results, until the proportion of water, produced by their combustion, is also accurately ascertained. With the view of effecting this, I have already spent much time, and employed many contrivances, none of which have satisfactorily answered the purpose for which they were intended.

7. There appears to be a considerable difference in the specific gravity and combustibility of gas from various specimens of coal, even when taken at similar periods of the distillation. The coal from Merthyr in South Wales, which burns without flame or smoke, yields a gas, which contains, in an equal volume, scarcely half as much combustible matter as the gas from Wigan cannel. This will probably be found to be the case with respect to all coal of similar quality,

Gas from coal burning without flame or smoke.

The more abundant the gas, the more it requires purification.

quality, among which may be reckoned the Kilkenny coal. The most important difference among the varieties of this mineral, connected with their application as sources of light, consists in the quantity of sulphuretted hidrogen gas, which is mixed with their aëriform products; and it unfortunately happens, that the coal, otherwise best adapted to this purpose, yields generally the largest proportion of this offensive gas. The only effectual method of purifying the coal gas from sulphuretted hidrogen, on the large scale of manufacture, will probably be found to consist in agitation with quicklime and water, composing a mixture of the consistence of cream. Simple washing with water by no means effects the complete separation.

Best mode of purifying it.

Condensible fluids neglected.

In the experiments which were made on the products of the distillation of coal, I purposely neglected the amount and analysis of the condensible fluids, because they cannot be advantageously ascertained by the same operation with the elastic ones. They may also be much better determined on the large scale of manufacture, than by limited experiments. For the same reason I was not solicitous to measure even the aëriform fluids; and on this subject, I believe, more accurate information has been communicated by Mr. Murdoch, than it was in my power to acquire.

Manchester, May 19, 1808.

III.

Account of a new irregularity lately perceived in the apparent Figure of the Planet Saturn. *By* WILLIAM HERSCHEL, *LL. D. F. R. S*[*].

Singular figure of Saturn.

THE singular figure of Saturn, of which I have given an account in two papers[†], has continued, for several reasons, to claim my attention. When I saw the uncommon flattening of the polar regions of this planet in the 40-feet telescope, I ascribed it to the attractive matter in the ring‡, and of its tendency to produce such an effect we can have no doubt; but as another circumstance, which was also noticed, namely, an apparent small flattening of the equatorial parts, cannot be explained on the same principles, I wished to ascertain what physical cause might be assigned for this effect, and with a view to an investigation of this point, I have continued my observations. The position of the ring, at the last appearance of the planet, however, proved to be quite unfavourable for the intended purpose; for the very parts, which I was desirous of inspecting, were covered by the passage of the ring over the disk of the planet in front, or were projected on the ring, where it passed behind the body.

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[*] Philos. Trans. for 1808, p. 159.

[†] See our Journal, vol. XIII, p. 4.

Not wholly explicable by the attraction of the ring.

In my attempts to pursue this object, I perceived a new irregularity in the Saturnian figure, which, I am perfectly assured, had no existence the last time I examined the planet, and the following observations contain an account of it.

Recent change in its figure.

Observations.

June 16, 1807. The two polar regions of Saturn are at present of a very different apparent shape. The northern regions, as in former observations, are flattened; but the southern are more curved or bulged outwards.

The two poles differ.

I asked my son John Herschel, who after me looked at Saturn while I was writing down the above observation, if he perceived, that there was a difference in the curvature of the north and south pole; and if he did, to mark on a slate how it appeared to him. When I examined the slate, I found that he had exactly delineated the appearance I have described.

In a letter to a very intelligent astronomical friend*, who has one of my 7-feet reflectors, I requested the favour of him to examine both the polar regions of Saturn, and to let me know whether he could perceive any difference in the appearance of their curvature; in answer to which I received, the 23d of June, a letter enclosing a drawing, in which also the southern regions were marked as more protuberant, with a greater falling off close to the irregularity. My friend, with his usual precaution, called this an illusion; and it will be seen by and by, that we shall have no occasion to ascribe this irregularity to a real want of due proportion, or settled figure of the polar regions of Saturn.

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‡ See Phil. Trans. for 1805, p. 276; or Journal, vol. xiii, p. 8.

* Dr. Wilson of Hampstead, late Professor of Astronomy at Glasgow.

June 22, 9h 24′. I see the same curved appearance at the south pole of Saturn, which was observed the 16th.

June 24. The air is very clear, and all the most critical phenomena are very distinctly to be seen; the shadow of the ring towards the south upon the planet; the shadow of the body towards the north-following side upon the ring; the belts upon the body; the division of the two rings; and with the same distinctness, I also see the protuberance of the south pole.

Not a distortion of some particular spot in the polar region.

My seeing this appearance, at present, is a proof, that it is not a physical irregularity or distortion of only some particular spot on the polar regions; for, in that case, it could not have been seen this evening, as from the rotation of the planet on its axis, which is 10h 16′, the space of the polar circle which is now exposed to our view must have been very different from what I saw the 16th and 22d.

Many observations were made afterward, which all confirm the reality of this appearance.

It is probably an illusion,

It is so natural for us to reflect upon the cause of a new phenomenon, that I cannot forbear giving an opinion on this subject. To suppose a real change in the whole zone of the planet, cannot be probable; it seems therefore, that this appearance must be, as my friend calls it, an illusion. But since the reality of this illusion, if I may use the expression, has been ascertained by observation, it is certain, that there must be some extrinsic cause for its appearance; and also that the same cause must not act upon the northern hemisphere. Now the only difference in the circumstances under which the two polar regions of Saturn were seen in the foregoing observations is the situation of its ring, which passes before the planet at the south, but behind at the north. The rays of light therefore, which come to the eye from the very small remaining southern zone of the saturnian globe, pass at no great distance by the edge of the ring, while those from the north traverse a space clear of every object that might disturb their course. If therefore we are in the right to ascribe the observed illusion to an approximate interposition of the ring, we have, in the case under consideration, only two known causes, that can modify light so as to turn it out of its course, which are inflection and refraction. The insufficiency of the first to account for the lifting up of the protuberant small segment of the southern regions will not require a proof. The effects of refraction on the contrary are known to be very considerable. Let us therefore examine a few of the particulars of the case. The greatest elevation of the visible segment above the ring did not amount to more than one second and three or four tenths. Then supposing the ring, the edge of which is probably of an elliptical figure, to have a surrounding atmosphere, it will most likely partake of the same form, and the rays which pass over its edge will undergo a double refraction: the first on their entrance into this atmosphere, and the second at their leaving it, and these refractions seem to be sufficient to produce the observed elevation. For should they raise the protuberant appearance only half a second, or even less, the segment could no longer range with the rest of the globe of Saturn, but must assume the appearance of a different curvature or bulge outwards.

owing to the position of the ring between the eye and this region,

the ring having an atmosphere by which the rays of light are refracted.

The refractive power of an atmosphere of the ring has been mentioned in a former paper*, when the smallest satellites of Saturn were seen as it were bisected by the narrow luminous lines under which form the ring appeared when the Earth was nearly in the plane of it; and the phenomenon, of which the particulars have now been described, appears to be a second instance in support of the former.

The refractive power of this atmosphere already shown.

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* See Phil. Tans. for 1790, page 7.

IV.

IV.

Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood. By THOMAS YOUNG, *M. D. For. Sec. R. S*[*].

I. *Of the Friction and Discharge of Fluids running in Pipes, and of the Velocity of Rivers.*

Motion of fluids in flexible elastic tubes.

HAVING lately fixed on the discussion of the nature of inflammation, for the subject of an academical exercise, I found it necessary to examine attentively the mechanical principles of the circulation of the blood, and to investigate minutely and comprehensively the motion of fluids in pipes, as affected by friction, the resistance occasioned by flexure, the laws of the propagation of an impulse through the fluid contained in an elastic tube, the magnitude of a pulsation in different parts of a conical vessel, and the effect of a contraction advancing progressively through the length of a given canal. The physiological application of the results of these inquiries I shall have the honour of laying before the Royal Society at a future time; but I have thought it advisable to communicate, in a separate paper, such conclusions, as may be interesting to some persons, who do not concern themselves with disquisitions of a physiological nature; and I imagine it may be as agreeable to the Society, that they should be submitted at present to their consideration, as that they should be withheld until the time appointed for the delivery of the Croonian Lecture.

Dubuat's calculations agree with experiment.

It has been observed by the late Professor Robison, that the comparison of the Chevalier Dubuat's calculations with his experiments is in all respects extremely satisfactory; that it exhibits a beautiful specimen of the means of expressing the general result of an extensive series of observations in an analytical formula, and that it does honour to the penetration, skill, and address of Mr. Dubuat, and of

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[*] Philos. Trans. for 1808, p. 164.

Mr. de St. Honoré, who assisted him in the construction of his expressions. I am by no means disposed to dissent from this encomium; and I agree with Professor Robison, and with all other late authors on hydraulics, in applauding the unusually accurate coincidence between these theorems and the experiments from which they were deduced. But I have already taken the liberty of remarking, in my lecture on the history of hydraulics, that the form of these expressions is by no means so convenient for practice as it might have been rendered: and they are also liable to still greater objections in particular cases, since, when the pipe is either extremely narrow, or extremely long, they become completely erroneous: for notwithstanding Mr. Dubuat seems to be of opinion, that a canal may have a finite inclination, and yet the water contained in it may remain perfectly at rest, and that no force can be sufficient, to make water flow in any finite quantity through a tube less than one twenty-fifth of an inch in diameter; it can scarcely require an argument to show, that he is mistaken in both these respects. It was therefore necessary for my purpose to substitute, for the formulæ of Mr. Dubuat, others of a totally different nature; and I could follow Dubuat in nothing but in his general mode of considering a part of the pressure, or of the height of a given reservoir, as employed in overcoming the friction of the pipe through which the water flows out of it; a principle, which, if not of his original invention, was certainly first reduced by him into a practical form. By comparing the experiments, which he has collected, with some of Gerstner, and some of my own, I have ultimately discovered a formula, which appears to agree fully as well as Dubuat's with the experiments from which his rules were deduced, which accords better with Gerstner's experiments, which extends to all the extreme cases with equal accuracy, which seems to represent more simply the actual operation of the forces concerned, and which is direct in its application to practice, without the necessity of any successive approximations.

The form of the expressions not very convenient for practice.

In some cases erroneous.

A formula discovered, that has advantages over Dubuat's.

I began by examining the velocities of the water discharged through pipes of a given diameter with different degrees

Velocities of water discharged through pipes with different pressures compared.

degrees of pressure; and I found, that the friction could not be represented by any single power of the velocity, although it frequently approached to the proportion of that power, of which the exponent is 1·8; but that it appeared to consist of two parts, the one varying simply as the velocity, the other as its square. The proportion of these parts to each other must however be considered as different in pipes of different diameters, the first part being less perceptible in very large pipes, or in rivers, but becoming greater than the second in very minute tubes; while the second also becomes greater for each given portion of the internal surface of the pipe, as the diameter is diminished.

Formula.

If we express, in the first place, all the measures in French inches, calling the height employed in overcoming the friction f, the velocity in a second v, the diameter of the pipe d, and its length l, we may make $f = a\frac{l}{d}v^2 + 2c\frac{l}{d}v$; for it is obvious, that the friction must be directly as the length of the pipe; and since the pressure is proportional to the area of the section, and the surface producing the friction to its circumference or diameter, the relative magnitude of the friction must also be inversely as the diameter, or nearly so, as Dubuat has justly observed. We shall then find that a must be $\cdot 0000001\left(430 + \frac{75}{d} - \frac{1440}{d+12} - \frac{180}{d+\frac{1}{3}}\right)$, and $c = \cdot 0000001\left(\frac{900dd}{dd+1000} + \frac{1}{\sqrt{d}}\left(1050 + \frac{12}{d} + \frac{\cdot 9}{dd}\right)\right)$.

Hence the velocity may easily be calculated for any pipe or river with any head.

Hence it is easy to calculate the velocity for any given pipe or river, and with any given head of water. For the height required for producing the velocity, independently of friction, is, according to Dubuat, $\frac{v^2}{478}$, or rather, as it appears from almost all the experiments which I have compared, $\frac{v^2}{550}$; and the whole height h is therefore equal to $f + \frac{v^2}{550}$, or $h = \left(\frac{al}{d} + \frac{1}{550}\right)v^2 + \frac{2cl}{d}v$; and making $b = \frac{1}{al \div d + \cdot 00182}$, and

$= \frac{bcl}{d}$, $v^2 + 2ev = bh$, whence $v = \surd\,(bh + e^2) - e$.

rder to adapt this formula to the case of rivers, we make l infinite; then b becomes $\frac{d}{al}$ and $bh = \frac{d}{a}\cdot\frac{h}{l}$

In rivers.

, s being the sine of the inclination, and d four times ydraulic mean depth; and since e is here $= \frac{c}{a}$, $v =$ $\frac{ds + cc) - c}{a}$, and in most rivers, v becomes nearly $\surd$ 0 ds).

Various experiments compared with the formula.

order to show the agreement of these formulæ with esult of observation, I have extracted, as indiscrimi-y and impartially as possible, forty of the experiments and collected by Dubuat; I have added to these some erstner's, with a few of my own; and I have compared esults of these experiments with Dubuat's calculations, with my own formulæ, in separate columns. There are f Dubuat's experiments, which he has rejected as irre-, apparently without any very sufficient reason, since s accidentally mentioned, that some of them were made great care: I have therefore calculated the velocities ese experiments in both ways, and compared the re-in a separate table.

Tabular

Tabular Comparison of Hydraulic Experiments.

Observer	d.	$\frac{1}{s}$	Superf. Veloc.	v.	Dub.	Log. ratio.	Y.	Log. ratio.	$\overset{a}{\cdot 1^{7}\times}$	$\overset{c}{\cdot 1^{7}\times}$	$\sqrt{(20000ds)}$
DUBUAT	262·5	35723	15·96	12·56 ?	10·33	·0776	11·10	·0537	424	952	11·1
	258·5	6413	51·77	26·65 ?	28·76	·0334	28·02	·0221	424	952	28·3
	92·4	21827	9·61	7·01 ?	8·38	·0775	8·14	·0649	415	914	9·3
	75 6	27648	7·27	5·07 ?	6·55	·1112	6·27	·0923	413	887	7·5
	17·6	9288		5·70	5·66	·0120	5·97	·0291	376	465	6·1
	16·4	432		32·52	31·61	·0124	30·67	·0255	374	451	27·6
	11·7	1412		14·17	13·59	·0182	14·05	·0087	360	416	12·2
	9·9	427		22·37	24·37	·0372	24·41	·0379	355	414	21·7
	5·8	212		27·51	27·19	·0051	27·34	·0027	332	466	23·5

Observers.	d.	l.	h.	v.	Dub.	Log ratio.	Y.	Log. ratio	$\overset{a}{\cdot 1^{7}\times}$	$\overset{c}{\cdot 1^{7}\times}$
COUPLET.	18	43200	145·08	39·16	40·51	·0148	38·49	·0075	376	469
	5	84240	25·00	5·32	5·29	·0024	5·40	·0065	326	492
			16·75	4·13	4·23	·0103	4·21	·0085		
			5·58	2·01	2·25	·0490	2·01	·0000		
BOSSUT.	2·01	2160	24	24·73	24·08	·0115	24·76	·0006	287	747
			12	16·38	16·10	·0075	16·86	·0125		
		1080	24	35·77	35·10	·0082	35·05	·0089		
		360	24	58·90	58·80	·0007	56·85	·0154		
	1·33	2160	12	12·56	12·75	·0065	13·28	·0242	270	919
		1080	24	28·08	28·21	·0020	28·84	·0116		
		560	24	48·53	49·52	·0088	48·66	·0015		
	1.	600	12	22·28	21·98	·0055	22·83	·0106	259	1063
			4	12·22	11·76	·0167	11·92	·0108		
DUBUAT.		737	23·7	28·67	29·41	·0111	30·11	·0213		
			12·2	19·99	19·95	·0009	20·67	·0145		
			4·2	10·56	10·66	·0041	10·90	·0137		
		117	36	84·95	85·32	·0029	83·12	·0069		
			18	58·31	58·47	·0014	58·41	·0012		
	·24167	36·25	53·25	85·77	85·20	·0029	85·71	·0003	309	2268
			41·25	73·81	73·90	·0005	74·67	·0050		
			20·17	51·96	50·14	·0155	50·87	·0093		
			5·00	23·40	23·19	·0039	23·09	·0058		
			·83	7·58	8·22	·0420	7·22	·0212		
	·1667	36·25	51·25	64·37	64·95	·0031	64·08	·0021	402	2827
			38·75	54·19	55·32	·0090	54·93	·0055		
			15·29	33·38	33·17	·0028	32·67	·0094		
			2·04	10·62	10·49	·0053	9·24	·0604		
	·125	34·17	42·17	45·47	46·21	·0070	45·88	·0039	518	3405
			35·33	41·61	41·71	·0010	41·55	·0006		
			14·58	26·20	25·52	·0114	24·94	·0214		
			2·08	7·32	8·35	·0572	6·98	·0206		
						(Mean ·0178 =L. 1·042)		(Mean ·0169 =L. 1·040)		

GERSTNER,

ervers	*d.*	*l.*	*h.*	*v.*	Dub.	Log. rat.	Y.	Log. rat.	*a.*	*c.*
ITNER, 5·3°F.	·2	63	10.7	24·2	23·9	·006	24·1	·002	349	2533
			7·7	21·0	19·9	·023	19·1	·042		
			4·7	15·8	14·9	·026	13·9	·056		
			1·7	7·5	8·2	·039	6·9	·036		
			·7	2·5	5·0	·301	3·4	·133		
	·138	33	10·7	27·1	23·4	·064	22·5	·081	488	3259
			7·7	23·2	19·4	·077	18·5	·098		
			4·7	15·4	14·6	·024	13·5	·058		
			1·7	5·6	8·1	·160	6·7	·078		
			·7	2·5	4·6	·301	3·4	·169		
	·0674	33	10·7	10·0	8·9	·051	10·1	·004	975	5700
			7·7	7·2	7·4	·012	8·2	·057		
			4·7	4·5	5·6	·095	5·6	·095		
			1·7	1·5	3·1	·316	2·5	·222		
			·7	·5	1·8	·444	1·1	·342		

(Mean ·129=L. 1·346 ·098=L. 1·254)

60°.	$\frac{1}{41}$	8·50	32·4	14·40	o	∞	13·36	·032	2956	18882
	$\frac{1}{185}$	3·42	30·0	·53			·52	·008	13404	452100
		1·17	5·8	·27			·30	·046		

(Mean ·029

AT.	2	255·25	36·35	86·31	84·2	·011	79·7	·035	287	747
	1	24	36·25	122·59	117·8	·018	120·8	·007	259	1060
			27	106·45	101·1	·022	104·1	·010		
			18	84·85	82·2	·013	84·8	·000		
			9	59·25	57·5	·013	59·7	·004		
	4		27·08	118·67	111·5	·027	118·5	·000		

(Mean ·017=L. 1·041 ·009=L. 1·022

Accuracy of the two formulæ compared.

appears from this comparison, that in the forty experi-
s extracted from the collection, which served as a basis
Dubuat's calculations, the mean errour of his formula
of the whole velocity, and that of mine $\frac{1}{25}$ only; but
omit the four experiments, in which the superficial
ity only of a river was observed, and in which I have
lated the mean velocity by Dubuat's rules, the mean
r of the remaining 36 is $\frac{1}{35}$, according to my mode of
lation, and $\frac{1}{37}$ according to Mr. Dubuat's; so that on
vhole, the accuracy of the two formulæ may be con-
ed as precisely equal with respect to these experiments.

In the six experiments which Dubuat has wholly rejected, the mean errour of his formula is about $\frac{1}{34}$, and that of mine $\frac{1}{45}$. In fifteen of Gerstner's experiments, the mean errour of Dubuat's rule is one third, that of mine one fourth; and in the three experiments which I made with very fine tubes, the errour of my own rules is one fifteenth of the whole, while in such cases Dubuat's formulæ completely fail. I have determined the mean errour by adding together the logarithmic ratios of all the results, and dividing the sum by the number of experiments. It would be useless to seek for a much greater degree of accuracy, unless it were probable, that the errours of the experiments themselves were less than those of the calculations; but if a sufficient number of extremely accurate and frequently repeated experiments could be obtained, it would be very possible, to adapt my formula still more correctly to their results.

In order to facilitate the computation, I have made a table of the coefficients a and c for the different values of d, all the measures being still expressed in French inches.

Table of Coefficients for French Inches.

d	$\overset{a}{\cdot 1^7 \times}$	$\overset{c}{\cdot 1^7 \times}$	d	$\overset{a}{\cdot 1^7 \times}$	$\overset{c}{\cdot 1^7 \times}$	d	$\overset{a}{\cdot 1^7 \times}$	$\overset{c}{\cdot 1^7 \times}$
00	430	900	15	370	427	·7	249	1278
500	427	943	10	354	414	·6	248	1384
400	426	946	9	350	421	·5	249	1524
300	423	950	8	345	433	·4	257	1717
200	421	951	7	340	440	$\frac{1}{3}$	268	1895
100	416	923	6	335	462	·3	279	2008
90	415	911	5	325	512	$\frac{1}{4}$	303	2225
80	413	896	4	319	540	·2	349	2532
70	410	872	3	305	617	$\frac{1}{6}$	402	2827
60	408	840	2·5	296	687	·15	440	3026
50	406	792	2	288	751	$\frac{1}{7}$	458	3116
40	400	719	1·5	275	866	$\frac{1}{8}$	518	3405
30	393	618	1	259	1063	$\frac{1}{9}$	589	3693
25	387	560	·9	255	1123	·1	646	3985
20	380	492	·8	252	1193			

Remarks on the coefficients.

For example, in the last experiment, where d is 1, l 4, and h 27·1, we have $a = \cdot 0000259$, $b = \frac{1}{al : d + \cdot 00182} = 516$, $c = \cdot 0001063$, $e = bcl : d = \cdot 22$, and $v = \surd\,(bh + e^2) - e = 118\cdot 46$, which agrees with the experiment within $\frac{1}{100}$ of the whole. I had at first employed for a the formula $\frac{430}{1 + 12 : d} + \frac{57}{d} + \frac{1}{6dd}$, but I found, that the value, thus determined, became too great when d was about 20, and too small in some other cases. Coulomb's experiments on the friction of fluids, made by means of the torsion of wires, give about ·00014 for the value of c, which agrees as nearly with this table, as any constant number could be expected to do. I have however reason to think, from some experiments communicated to me by Mr. Robertson Buchanan, that the value of a, for pipes about half an inch in diameter, is somewhat too small; my mode of calculation, as well as Dubuat's, giving too great a velocity in such cases.

Assistants to Dubuat's formula.

If any person should be desirous of making use of Dubuat's formula, it would still be a great convenience to begin by determining v according to this method; then, taking $b = \frac{l}{h - v^2 : 478}$, or rather, as Langsdorf makes it, $b = \frac{l}{h - v^2 : 482}$, to proceed in calculating v by the formula $v = 148\cdot 5\,(\surd d - \cdot 2) \cdot \left(\frac{1}{\surd b - \text{H. L.}\,\surd\,(b + 1\cdot 6)} - \cdot 001\right)$, since this determination of b will, in general, be far more accurate than the simple expression $b = \frac{l + 45d}{h}$, and the continued repetition of the calculation, with approximate values of v, may thus be avoided. Sometimes, indeed, the values of v found by this repetition will constitute a diverging instead of a converging series, and in such cases, we can only employ a conjectural value of v, intermediate between the two preceding ones.

The formula reduced to English measure.

Having sufficiently examined the accuracy of my formula, I shall now reduce it into English inches, and shall add a second table of the coefficients, for assisting the calculation. In this case, a becomes $\cdot 0000001 \left(413 + \frac{75}{d} - \frac{1440}{d + 12\cdot 8} - \frac{180}{d + \cdot 355}\right)$, $c = \cdot 0000001 \left(\frac{900dd}{dd + 1136} + \frac{1}{\sqrt{d}} \left(1085 + \frac{13\cdot 21}{d} + \frac{1\cdot 0563}{dd}\right)\right)$, and $b = \frac{1}{al : d + \cdot 00171}$, e being $\frac{bcl}{d}$, and $v = \sqrt{}\ (bh + e^2) - e$, or $= \sqrt{}\ \left(\frac{ds}{a} + \frac{ee}{aa}\right) - \frac{e}{a}$, as before; and in either case the superficial velocity of a river may be found, very nearly, by adding to the mean velocity v its square root, and the velocity at the bottom by subtracting it.

Table of Coefficients, for English Inches.

d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$	d	a $\cdot 1^7 \times$	c $\cdot 1^7 \times$
00	413	900	15	354	430	·7	243	1322
500	410	944	10	339	413	·6	243	1433
400	409	948	9	336	421	·5	245	1578
300	406	951	8	331	433	·4	254	1779
200	404	951	7	327	449	$\frac{1}{3}$	268	1963
100	399	918	6	322	471	·3	280	2082
90	398	903	5	312	507	$\frac{1}{4}$	305	2307
80	396	885	4	306	556	·2	354	2631
70	393	860	3	292	635	$\frac{1}{6}$	409	2943
60	391	825	2·5	284	694	·15	447	3150
50	389	772	2	277	774	$\frac{1}{7}$	466	3251
40	383	698	1·5	266	894	$\frac{1}{8}$	528	3558
30	377	597	1	251	1099	$\frac{1}{9}$	599	3866
25	371	526	·9	248	1161	·1	657	4183
20	364	482	·8	245	1234			

II. *Of the Resistance occasioned by Flexure in Pipes or Rivers.*

Dubuat's experiments on effect of flexure in a pipe.

Mr. Dubuat has made some experiments on the effect of the flexure of a pipe in retarding the motion of the water flowing through it; but they do not appear to be by any means sufficient to authorise the conclusions, which he has drawn from them. He directs the squares of the sines of the angles of flexure to be collected into one sum, which, being multiplied by a certain constant coefficient, and by the square of the velocity, is to show the height required for overcoming the resistance. It is, however, easy to see, that such a rule must be fundamentally erroneous, and its coincidence with some experiments merely accidental, since the results afforded by it must vary according to the method of stating the problem, which is entirely arbitrary. Thus it depended only on Mr. Dubuat to consider a pipe bent to an angle of 144° as consisting of a single flexure, as composed of two flexures of 72° each, or of a much greater number of smaller flexures; although the result of the experiment would only agree with the arbitrary division into two parts, which he has adopted. This difficulty is attached to every mode of computing the effect either from the squares of the sines or from the sines themselves; and the only way of avoiding it is to attend merely to the angle of flexure as expressed in degrees. It is natural to suppose, that the effect of the curvature must increase, as the curvature itself increases, and that the retardation must be inversely proportional to the radius of curvature, or very nearly so; and this supposition is sufficiently confirmed by the experiments which Mr. Dubuat has employed in support of a theory so different. It might be expected, that an equal curvature would create a greater resistance in a larger pipe than in a smaller, since the inequality in the motions of the different parts of the fluid is greater; but this circumstance does not seem to have influenced the results of the experiments made with pipes of an inch and of two inches diameter: there must also be some deviation from the general law in cases of very small pipes having a great curvature, but this deviation cannot be determined without farther experiments. Of the 25 which Dubuat has made, he has rejected 10 as irregular, because they do not agree with his theory: indeed 4 of them, which were made with a much shorter pipe than the rest, differ so manifestly from them, that they cannot be reconciled: but 5 others agree sufficiently, as well as all the rest, with the theory which I have here proposed, supposing

His rule

fundamentally erroneous.

A different theory.

Sufficiently confirmed by experiments.

the resistance to be as the angular flexure, and to increase besides almost in the same proportion as the radius of curvature diminishes, but more nearly as that power of the radius of which the index is $\frac{4}{5}$. Thus if p be the number of degrees subtended at the centre of flexure, and q the radius of curvature of the axis of the pipe in French inches, we shall have $r = \frac{p v^2}{209000 q}$ nearly, or, more accurately, $r = \frac{\cdot 0000045 \, p v^2 q^{\frac{1}{5}}}{q}$. These calculations are compared with the whole of Dubuat's experiments in the following table.

Table of Experiments on the Resistance occasioned by Flexure.

p	q	v^2	r	B.	Y. 1	Y. 2
288	3.22	15030	4·75		6·71	6·98
		11330	3·50		5·06	5·26
		7199	2·33		3·21	3·34
		3510	1·08		1·56	1·62
216		7216	2·49	2·49	2·42	2·52
144			1·50	1·66	1·61	1·67
72			·75	·83	·80	·83
196·5	6·12		1·50	1·66	1·16	1·31
147·4			1·12	1·24	·87	·98
98·3			·75	·83	·58	·65
49·1			·37	·41	·29	·33
112·5	·53		6·00		7·68	6·36
99			5·90		6·74	5·60
288	3·22	3415	1·50	1·57	1·52	1·58
288	3·22	3415	1·50	1·57	1·52	1·58
144			·75	·78	·76	·79
72			·37	·39	·38	·39
196·5	6·12		·75	·78	·55	·62
112·5	·53		1·50		3·63	3·00
720	3·22	5125	5·90	5·90	5·72	5·95
288		3458	1·64	1·59	1·54	1·60
		860	·41	·40	·38	·40
—		821	·39	·38	·37	·38
288	4·10	3448	1·33		1·21	1·30
		7449	2·90		2·59	2·78
294·8 360 112·5	9·9 4·1 1·1		8·64		8·08	8·62

In

Remarks.

In the last three experiments, the diameter of the pipe was two inches. The radius of curvature is not ascertained within the tenth of an inch, as Dubuat has not mentioned the thickness of the pipes. The mean errour of his formula in fifteen experiments, and of mine in twenty, is $\frac{1}{25}$ of the whole.

III. *Of the Propagation of an Impulse through an Elastic Tube.*

Propagation of an impulse through an elastic tube.

The same reasoning, that is employed for determining the velocity of an impulse, transmitted through an elastic solid or fluid body, is also applicable to the case of an incompressible fluid contained in an elastic pipe; the magnitude of the modulus being properly determined, according to the excess of pressure which any additional tension of the pipe is capable of producing; its height being such, as to produce a tension, which is to any small increase of tension produced by the approach of two sections of the fluid in the pipe, as their distance to its decrement: for in this case the forces concerned are precisely similar to those, which are employed in the transmission of an impulse through a column of air enclosed in a tube, or through an elastic solid.

Propositions.

If the nature of the pipe be such, that its elastic force varies as the excess of its circumference or diameter above the natural extent, which is nearly the usual constitution of elastic bodies, it may be shown, that there is a certain finite height which will cause an infinite extension, and that the height of the modulus of elasticity, for each point, is equal to half its height above the base of this imaginary column; which may therefore be called with propriety the modular column of the pipe: consequently the velocity of an impulse will be at every point equal to half of that which is due to the height of the point above the base; and the velocity of an impulse ascending through the pipe being every where half as great as that of a body falling through the corresponding point in the modular column, the whole time of ascent will be precisely twice as great as that of the descent of the falling body; and in the same manner if the pipe be inclined, the motion of the impulse may be compared with that of a body descending or ascending freely along an inclined plane.

Demonstration. These propositions may be thus demonstrated: let a be the diameter of the pipe in its most natural state, and let this diameter be increased to b by the pressure of the column c, the tube being so constituted, that the tension may vary as the force. Then the relative force of the column c is represented by bc, since its efficacy increases, according to the laws of hydrostatics, in the ratio of the diameter of the tube; and this force must be equal, in a state of equilibrium, to the tension arising from the change from a to b, that is, to $b-a$; consequently, the height c varies as $\frac{b-a}{b}$; and if the tube be enlarged to any diameter x, the corresponding pressure required to distend it will be expressed by a height of the column equal to $\left(1-\frac{a}{x}\right)\cdot\frac{bc}{b-a}$ since $\frac{b-a}{b}:c::\frac{x-a}{x}:\left(1-\frac{a}{x}\right)\frac{bc}{b-a}$. Now if the diameter be enlarged in such a degree, that the length of a certain portion of its contents may be contracted in the ratio $1:1-r$, r being very small, then the enlargement will be in the ratio $1:1+\frac{r}{2}$, that is, x' will be $\frac{rx}{2}$; but the increment of the force, or of the height, is $\frac{ax'}{xx}\cdot\frac{bc}{b-a}$, which will become $\frac{ar}{2x}\cdot\frac{bc}{b-a}$. Now in a tube filled with an elastic fluid, the height being h, the force in similar circumstances would be rh, and if we make $h=\frac{a}{2x}\cdot\frac{bc}{b-a}$, the velocity of the propagation of an impulse will be the same in both cases, and will be equal to the velocity of a body which has fallen through the height $\frac{1}{2}h$. Supposing x infinite, the height capable of producing the necessary pressure becomes $\frac{bc}{b-a}$, which may be called g, and for every other value of x this height is $\left(1-\frac{a}{x}\right)g$, or $g-\frac{ag}{x}$, or, since h becomes

$\frac{ag}{2x}$, $g - 2h$, so that h is always equal to half the difference between g and the actual height of the column above the given point, or to half the height of the point above the base of the column.

If two values of x, with their corresponding heights, are given, as b and x, corresponding to c and d, and it is required to find a; we have $\frac{b-a}{b} : c :: \frac{x-a}{x} : d$, $dbx - dax = cbx - cba$, and $a = \frac{dbx - cbx}{dx - ch}$, or $\frac{b}{a} = \frac{dx - ch}{dx - cx}$. Thus if the height equivalent to the tension vary in the ratio of any power m of the diameter, so that, n being a small quantity, $x = b\,(1 + n)$ and $d = c\,(1 + mn)$, $\frac{b}{a} = \frac{bc\left((1+n).(1+mn) - 1\right)}{bc\left((1+n).(1+mn - (1+n)\right)} = \frac{mn + n}{mn}$, since the square of n is evanescent, and $\frac{b}{a} = \frac{m+1}{m}$. For example, if $m = 4$, $\frac{b}{a} = \frac{5}{4}$, and if $m = 2$, $b : a :: 3 : 2$.

IV. *Of the Magnitude of a diverging Pulsation at different Points.*

Magnitude of a diverging pulsation at different points.

The demonstrations of Euler, Lagrange, and Bernoulli, respecting the propagation of sound, have determined, that the velocity of the actual motion of the individual particles of an elastic fluid, when an impulse is transmitted through a conical pipe, or diverges spherically from a centre, varies in the simple inverse ratio of the distance from the vertex or centre, or in the inverse subduplicate ratio of the number of particles affected, as might naturally be inferred from the general law of the preservation of the ascending force or impetus, in all cases of the communication of motion between elastic bodies, or the particles of fluids of any kind. There is also another way of considering the subject, by which a similar conclusion may be formed respecting waves diverging from or converging to a centre. Suppose a straight

Waves.

wave to be reflected backwards and forwards in succession, by two vertical surfaces, perpendicular to the direction of its motion; it is evident that in this and every other case of such reflections, the pressure against the opposite surfaces must be equal, otherwise the centre of inertia of the whole system of bodies concerned would be displaced by their mutual actions, which is contrary to the general laws of the properties of the centre of inertia. Now if, instead of one of the surfaces, we substitute two others, converging in a very acute angle, the wave will be elevated higher and higher as it approaches the angle: and if its height be supposed to be every where in the inverse subduplicate ratio of the distance of the converging surfaces, the magnitude of the pressure, reduced to the direction of the motion, will be precisely equal to that of the pressure on the single opposite surface, which will not happen if the elevation vary inversely in the simple ratio of the distance, or in that of any other power than its square root. This mode of considering the subject affords us therefore an additional reason for asserting, that in all transmissions of impulses through elastic bodies, or through gravitating fluids, the intensity of the impulse varies inversely in the subduplicate ratio of the extent of the parts affected at the same time; and the same reasoning may without doubt be applied to the case of an elastic tube.

Intensity of the impulse varies inversely in the subduplicate ratio of the extent affected.

Waves crossing each other.

There is however a very singular exception, in the case of waves crossing each other, to the general law of the preservation of ascending force, which appears to be almost sufficient to set aside the universal application of this law to the motions of fluids. It is confessedly demonstrable, that each of two waves, crossing each other in any direction, will preserve its motion and its elevation with respect to the surface of the fluid affected by the other wave, in the same manner as if that surface were plane; and, when the waves cross each other nearly in the same direction, both the height and the actual velocity of the particles being doubled, it is obvious, that the ascending force or impetus is also doubled, since the bulk of the matter concerned is only halved, while the square of the velocity is quadrupled; and supposing the double wave to be stopped by an obstacle, its magnitude, at the moment of the greatest elevation, will be twice as great as that of a single wave in similar circumstances, and the height, as well as the quantity of matter, will be doubled, so that either the actual or the potential height of the centre of gravity of the fluid seems to be essentially altered, whenever such an interference of waves takes place. This difficulty deserves the attentive consideration of those, who shall attempt to investigate either the most refined parts of hydraulics, or the metaphysical principles of the laws of motion.

V. *Of the Effect of a Contraction, advancing through a Canal.*

End of a rectangular horizontal velocity partly filled with water advancing with a given velocity.

If we suppose the end of a rectangular horizontal canal, partly filled with water, to advance with a given velocity, less than that with which a wave naturally moves on the surface of the water, it may be shown, that a certain portion of the water will be carried forwards, with a surface nearly horizontal, and that the extent of this portion will be determined, very nearly, by the difference of the spaces described, in any given time, by a wave, moving on the surface thus elevated, and by the movable end of the canal. The form of the anterior termination of this elevated portion, or wave, may vary, according to the degrees by which the motion may be supposed to have commenced; but whatever this form may be, it will cause an accelerative force, which is sufficient to impart successively to the portions of the fluid, along which it passes, a velocity equal to that of the movable end, so that the elevated surface of the parts in motion may remain nearly horizontal: and this proposition will be the more accurately true, the smaller the velocity of the movable end may be. For, calling this velocity v, the original depth a, the increased depth x, and the velocity of the anterior part of the wave y, we have, on the supposition that the extent of the wave is already become considerable, $x = \frac{ay}{y \mp v}$, taking the negative or positive sign according to the direction of the motion of the end; since the quantity of fluid, which before occupied a length expressed by y, now occupies the length $y \mp v$; and putting $a \backsim x = z$, $z = \frac{av}{y \mp v}$. The direction of the surface of the margin of the wave is indifferent to the calculation, and it is most convenient to suppose its inclination equal to half a right angle, so that the accelerating force, acting on any thin transverse vertical lamina, may be equal to its weight: then the velocity y must be such, that while the inclined margin of the wave passes by each lamina, the lamina may acquire the velocity v by a force equal to its own weight; consequently the time of its passage must be equal to that in which a body acquires the velocity v, in falling through a height b, corresponding to that velocity: and this time is expressed by $\frac{2b}{v}$; but the space described by the margin of the wave is not exactly z, because the lamina in question has moved horizontally during its acceleration, through a space which must be equal to b; the distance actually described will therefore be $z \pm b$, and we have $\frac{z \pm b}{y} = \frac{2b}{v}$, $z \pm b = \frac{2by}{v}$, $av \pm by - bv = \frac{2byy}{v} \mp 2by$, $y^2 \mp \frac{1}{2}vy = \frac{av^2}{2b} - \frac{v^2}{2}$, $(y \mp \frac{1}{4}v)^2 = \frac{av^2}{2b} + \frac{v^2}{16}$; but, m being the proper coefficient, $v = m\surd b$, and $v^2 = m^2 b$, $\frac{av^2}{2b} + \frac{v^2}{16} = m^2 \left(\frac{a}{2} + \frac{b}{16}\right)$, $y, = m\surd\left(\frac{a}{2} + \frac{b}{16}\right) \pm \frac{1}{4}v$, and $y \mp v = m\surd\left(\frac{a}{2} + \frac{b}{16}\right) \mp \frac{1}{4}v$. But when v is small, we may take $y \mp v$ nearly $m\surd\frac{a}{2}$, and $z = \frac{ma\surd b}{m\surd(\frac{1}{2}a)} = \surd(2ab)$, and $x = a \pm \surd(2ab)$, while the height of a fluid, in which the velocity would be y, is nearly $a \pm \frac{1}{2}\surd(2ab)$: consequently, when the velocity v is at all considerable, y must be somewhat greater than the velocity of a wave moving on the surface of the elevated fluid; and probably the surface of the elev-

ated portion will not in this case by perfectly horizontal;
where v is small, y may be taken, without material er-
r, $m \surd \frac{x}{2}$, or even $m \surd \frac{a}{2}$, which is the velocity of
ry small wave. The coefficient m is here assumed the
ie for the motion of a wave, as for the discharge through
aperture, and I have reason from observation to think
estimation sufficiently correct.

Case where the moving end remains open below.

upposing now the movable end of the canal to remain
n at the lower part as far as the height c, then the ex-
s of pressure, occasioned by the elevation before it, and
depression behind, will cause the fluid, immediately be-
the movable plane, to flow backwards, with the velocity
ermined by the height, which is the difference between
levels; and the quantity thus flowing back, together
h that which is contained in the movable elevation, must
equal to the whole quantity displaced. But the depres-
n, behind the movable body, must vary according to the
cumstances of the canal, whether it be supposed to end
uptly at the part from which the motion begins, or to be
tinued backwards without limit: in the first case, the
vation z will be to the depression as v to $y-v$, the length
the same portion of the fluid being varied inversely in
t ratio; in the second case, the proportion will be as y
v to $y-v$: and the difference of the levels will be $z+z$
$\frac{-v}{} = \frac{zy}{v}$, or secondly $z + z\frac{y-v}{y+v} = \frac{2zy}{y+v}$; and first, $m \surd$
$c + (y-v)z = (a-c)v$; but, since y is here consi-
ed as equal to $m \surd \frac{a}{1}$ putting $\surd \frac{a}{2} - \surd b = d$, $y-v$
$m d$, and, calling $a-c$, e, $m \surd \frac{zy}{v} c + m d z = m e \surd$
$\surd \frac{zy}{v} c + dz = e \surd b$, $c^2 \frac{zy}{v} = e^2b + d^2z^2 - 2dze \surd$
$z^2 - \left(\frac{c^2y}{d^2v} + \frac{2e\surd b}{d}\right) z = -\frac{e^2b}{d^2}$, and, calling $\frac{c^2y}{2d^2v} +$
$\frac{\ b}{}$, f, $z = f - \surd\left(f^2 - \frac{e^2b}{d^2}\right)$: and in the same manner

f is found, for the second case, equal to $\frac{c^2 y}{d^2 (y + v)} + \frac{e \sqrt{b}}{d}$. For example, suppose the height a 2 feet, $b = \frac{1}{4}$, $c = 1$, and consequently $e = 1$, then d becomes $\frac{1}{2}$, $v = 4$, and $y = 8$; and in the first case $z = \cdot 1$, and in the second $z = \cdot 14$.

Varieties of the velocity and open space.

If v, the velocity of the obstacle, were great in comparison with $m \sqrt{\frac{a}{2}}$, the velocity of a wave, and the space c below the obstacle were small, the anterior part of the elevation would advance with a velocity considerably greater than the natural velocity of the wave: but if the space below the obstacle bore a considerable proportion to the whole height, the elevation z would be very small, since a moderate pressure would cause the fluid to flow back, with a sufficient velocity, to exhaust the greatest part of the accumulation, which would otherwise take place. Hence the elevation must always be less than that which is determined by the equation $m \sqrt{z}\, c = e\, v$, and z is at most equal to $\left(\frac{e\, v}{m\, c}\right)^2 = \frac{e^2}{c^2} b$; but since the velocity of the anterior margin of the wave can never materially exceed $m \sqrt{\frac{x}{2}}$, especially when z is small, and $\sqrt{\frac{x}{2}}$ being in this case nearly $\sqrt{\frac{a}{2}} + \frac{e^2}{2 \sqrt{(\frac{1}{2} a)}\, c^2} b$, $m \sqrt{\frac{x}{2}} - m \sqrt{b} = m \left(\sqrt{\frac{a}{2}} + \frac{e^2 b}{\sqrt{(2a)}\, c^2} - \sqrt{b} \right)$ which, multiplied by z, shows the utmost quantity of the fluid that can be supposed to be carried before the obstacle. Supposing $b = \frac{1}{4} a$, this quantity becomes $m \sqrt{\frac{a}{2}} \cdot \frac{e^4}{c^4} \cdot \frac{a}{4}$; and if $\frac{e}{c}$ be, for example, $\frac{1}{10}$, it will be expressed by $\frac{1}{40000} a\, v$, while the whole quantity of the fluid left behind.

A contraction moving along an elastic pipe.

A similar mode of reasoning may be applied to other cases of the propagation of impulses, in particular to that of a contraction moving along an elastic pipe. In this case, an increase of the diameter does not increase the velocity of the transmission of an impulse; and when the velocity of the contraction approaches to the natural velocity of an impulse, the quantity of fluid protruded must, if possible, be still smaller than in an open canal; that is, it must be absolutely inconsiderable, unless the contraction be very great in comparison with the diameter of the pipe, even if its extent be such as to occasion a friction, which may materially impede the retrograde motion of the fluid. The application of this theory to the motion of the blood in the arteries is very obvious, and I shall enlarge more on the subject when I have the honour of laying before the Society the Croonian Lecture for the present year.

Motion of the blood.

Resistance to a floating body.

The resistance, opposed to the motion of a floating body, might in some cases be calculated in a similar manner: but the principal part of this resistance appears to be usually derived from a cause which is here neglected; that is. the force required to produce the ascending, descending, or lateral motions of the particles which are turned aside to make way for the moving body; while in this calculation their direct and retrograde motions only are considered.

Velocity of a wave.

The same mode of considering the motion of a vertical lamina may also be employed for determining the velocity of a wave of finite magnitude. Let the depth of the fluid be a, and suppose the section of the wave to be an isosceles triangle, of which the height is b, and half the breadth c: then the force urging any thin vertical lamina in a horizontal direction will be to its weight as b to c; and the space d, through which it moves horizontally, while half the wave passes it, will be such that $(c-d)\cdot(a+\frac{1}{2}b)=ac$, when $ced=\frac{bc}{2a+b}$. But the final velocity in this space is the same as is due to a height equal to the space, reduced in the ratio of the force to the weight, that is, to the height $\frac{bb}{2a+b}$, and half this velocity is $\frac{1}{2}m\sqrt{\left(\frac{bb}{2a+b}\right)}$, which is the mean velocity of the lamina. In the mean time the wave describes the space $c+d$, and its velocity is greater than that of the lamina in the ratio of $\frac{c}{d}+1$ to 1, that is $\frac{2a+b}{b}+1$ or $\frac{2a}{b}+2$ to 1, becoming $m\left(\frac{a}{b}+1\right)$

b

$\frac{b}{\sqrt{(2a+b)}} = m\frac{a+b}{\sqrt{(2a+b)}}$; which, when b vanishes, becomes $m\sqrt{\frac{a}{2}}$, as in Lagrange's theorem, and, when b is small, $m\left(\sqrt{\frac{a}{2}}+\frac{3}{4}\frac{b}{\sqrt{(2a+b)}}\right)$, or $m\frac{a+\frac{3}{4}b}{\sqrt{(2a)}}$; but if a were small, it would approach to $m\sqrt{b}$, the velocity due to the whole height of the wave.

V.

A Mineralogical Description of the Mountain and Silver Mine of Chalanches, in the Department of the Isère. By HERICART DE THURY, *Mine Engineer*[*].

Mountain of Chalanches.

THE mountain of Chalanches, noted in the annals of mineralogy for the variety and beauty of the mineral substances met with in it, is now become celebrated in metallurgy, for the abundance and richness of its silver ores.

Situation and height.

This mountain is in the vicinity of Allemont, in the canton of l'Oisans[†]. It is above the confluence of the Olle and the Romanche, 2 myriam. [about 12 miles] east of Grenoble in a straight line. Its height, taken at the buildings belonging to the silver mines, is 2159 met. [2359 yards] above the level of the sea; but some primitive peaks, stretching from south-east to north-west. rise 580 or 590 met. [635 or 645 yards] higher. Its loftiest summit is about 2750 met. [3005 yards].

Noticed by several, but imperfectly.

Many celebrated mineralogists have written on this mountain. Schreiber, director of the practical school of Mont-Blanc, who superintended the working of the silver mines at Chalanches several years with great success, has published many accurate and judicious observations on them in the Journal de Physique, but he has considered them merely as silver mines. De Bournon, in his mineralogy of Dauphiny, in the same Journal, has attempted to explain the origin of its veins, and given a very ingenious theory for this purpose; but unfortunately it is not applicable to the minerals of Chalanches. Dolomieu, Faujas de Saint-Fond, Dietrich, Mongez, Guettard, and others, have also spoken of this mountain, but no one has given a general view of its mineral substances.

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[*] Abridged from the Journal des Mines, vol. XX, p. 41.

[†] The canton of l'Oisans is the richest country in France with respect to its mineral substances. Its lofty mountains conceal a great number of veins, the various and superb products of which are daily enriching our finest collections.

De Bournon's theory of its veins inconsistent with facts.

Before I enumerate and describe the various products collected on the mountain of Chalanches, I shall give a geological sketch of it. It is of the primitive order, and composed of rocks, the base of some of which is simple, of others mixed. The latter are the most numerous, and constitute the chief mass of the mountain. The greater part of the veins yet known are found in a micaceous quartz rock, the strata of which dip in general to the south-west, but at an angle that frequently varies. The manner in which these rocks lie with respect to each other is pretty constant. Granite forms the base of the mountain, and is foliaceous. It frequently partakes of the nature of gneiss, sometimes of that of amphibolic rocks, and frequently of both at the same time. The gneiss and micaceous rocks vary as much in their grain and texture, as in the difference of their constituent elements. These rocks frequently alternate with amphibolic rocks; often they are mixed together; and still more commonly their association presents itself with all the characters of a granite, in which the mica abounds but little. In some places the gneiss contains iron pyrites, and occasionally calcareous particles, the presence of which is easily discovered by means of nitric acid. The colour of the gneiss varies extraordinarily. Gray, yellow, green, white, black, &c., are its most common tints: but it is frequently of a red, or reddish colour, whence the rocks it forms have been termed burnt.

Geological sketch of it.

Granite.

Gneiss.
Hornblende.

About a third of the way up the mountain, and 800 met. [874 yards] west of Traverse, the last hamlet we meet with before we get into the wood of the mine, we see in the road three strata of primitive limestone, which alternate with granitic,

Primitive limestone.

Feldspar and gneiss with garnets.
Granite with tourmalines.
Secondary limestone.

granitic, micaceous, and amphibolic rocks. These strata dip to the west at an angle of 60°. Their direction is north and south. This carbonate of lime is saccharoid, and emits a fetid smell when struck. Two of the three strata are a tolerably pure white; the third is gray with a rosy tinge, and adheres to the amphibole, that serves it as a wall. A little distance from this we find white feldspar rocks containing garnets; a little farther, gneiss with garnets; below, granitic rocks with tourmalines; and lastly, proceeding down to Allemont, the proper position of the secondary limestone on the primitive rocks,

Bent strata.

Toward the summit of the mountain we see quartzose and amphibolic rocks in strata that are bent, and turned back on themselves. Sometimes the doublings and redoublings are very numerous in the same mass.

Summit.

The summit of the mountain is schistose amphibole veined with quartz. It is naked, and in part destroyed, having fallen down in irregular blocks of different sizes.

Height.

The centre of the works is 1514 met. [1654 yards] above the confluence of the Romanche and Olle, or 2159 met. [2359 yards] above the level of the sea.

Ores.

The ores of Chalanches are disposed in veins, beds, and nodules; but these different modes are not continued on regularly.

Veins very irregular.

The veins are infinitely varied. Their magnitude, direction, and inclination vary continually, and are subject to numerous accidents. These veins are in general placed one above another; they are sometimes very near together; they cross each other in every direction; they preserve no regularity either in their course or dip; they frequently proceed in a direction opposite to what they took at first; lastly, they sometimes unite, and proceed together for a certain space, after which they separate, perhaps to unite again, or to disappear entirely, and with very different circumstance. The richness of the veins is not more constant than thei mode of being. Frequently we find veins yielding 20 or 25 parts of silver in 100 of ore, which at a few decimetres [the dec. is near 4 inches Eng.] distance present nothing but a sterile gangue.

Beds the same.

The beds of ore are not so common as the veins; they do not continue for any considerable length; and they experience the same accidents. Their richness, direction, inclination, thickness, &c., are continually varying. They are incessantly intersected, turned aside, choaked, and interrupted by the veins. Finally, as they appear to owe their formation to the same cause as produced and formed the veins after the rupture and convulsions of the mountain, and the filling up of its clefts, I am induced to consider them rather as horizontal veins, than as real beds.

Nodules.

Sometimes the ore occurs in nodules, but less frequently than in veins, and like these they vary both in richness and magnitude.

Gangue very various.

Their gangue is still more varied. Most commonly it is carbonate of lime, pure, mixed, associated, crystallized, amorphous, &c. Sometimes it is sulphate of lime; in other places it is flexible asbestus: frequently it is hyaline quartz, pure, mixed, crystallized, amorphous, &c.: occasionally it is argilaceo-calcareous: here it is green or brown pulverulent chrolite talc, and yonder the same in mass containing native silver: frequently it is epidote in mass or crystallized: &c. Among the metallic gangues we find oxide of cobalt both earthy and vitreous, arsenicated, arsenical, and gray cobalt, all of them more or less argentiferous. Arsenical and carbonated nickel frequently perform the office of a gangue, and sometimes the first of these is even rich in silver. Arsenic too is found in the argentiferous ores, but it is more rare than the preceding. Antimony occurs native, sulphuretted, oxided, and as a hidrosulphuret. Copper, which is very abundant in the veins, is met with in different states: pyritous, sulphuretted, oxided, and hidrosulphuretted. Gray copper ore is frequently found in the asbestus, and always very rich in silver. Iron and manganese, both in the state of oxide, occur very commonly in the veins at Chalanches: and the latter is even one of the richest gangues in silver. Lead is found as a sulphuret, and sometimes phosphated. Mercury too is found in the gangues at Chalanches, and frequently is even abundant in them.

Metallic gangues.

Valleys.

Above the silver mines we find two narrow valleys, one of which runs east, the other west. These narrow gradually to the foot of the highest peaks.

Anthracite.

The western valley, called the Clos du Chevalier, exhibits an interesting object of study, a stratum of anthracite between strata of clay schist with vegetable impressions deposited on a granitoid breccia. The latter immediately covers the primitive rocks of gneiss or amphibole, that contain the veins of silver.

High ridge.

Lastly, on the north of Chalanches is a sharp ridge, that joins the loftiest peaks of the great chain of Belledonne, which extends from Vizille to Allevard as you ascend toward the high mountains of Maurienne. This chain is distinguished for the number and variety of its rich metallic veins.

Chalanches an interesting object of study.

The geology of the mountain of Chalanches offers us grand facts, and various subjects of observation with respect to the catastrophes, that have destroyed and overturned its primitive organization; the violence it has subsequently undergone; the number of its veins, their formation, and continual changes; and the assemblage of so many various substances, separate or combined together, and modified in numberless ways. Of these substances I shall proceed to give a brief methodical sketch, arranged according to the four grand divisions of the system of Haüy.

Sketch of its minerals.

Haüy's 1st class. Carbonate of lime.

In the first class, acidiferous earthy substances, we have carbonate and sulphate of lime. The carbonate is frequently found crystallized, and exhibits numerous varieties of form and colour. It is still more various in its combinations. Some of its compounds with iron and manganese have been wrought as sparry iron ore. It is mixed with magnesia in large irregular crystals disseminated through steatitic, asbestous, chloritic, and magnesian gangues.

Sulphate of lime.

The sulphate has often been found in transparent crystals, sometimes coloured by copper, and lying on a silky amianthus, partly white, and partly rosecoloured from a mixture of pulverulent arseniate of cobalt. These are uncommon, and varied in a pleasing manner.

Sulphate of barytes.

At the foot of the mountain, toward Allemont, is a vein of sulphate of barytes, which was formerly explored in search of sulphuret of lead, that appeared there to the day. It has some varieties of form and colour, being radiated, granular, compact, and white.

Of

2d class. Quartz. Of the second class, earthy substances not acidiferous, quartz is tolerably abundant. It has been remarked, that its presence is a bad omen with regard to the richness of the silver ore. Its most usual combinations are with cobalt and antimony. Rock crystal. The hyaline quartz is pretty frequent in the veins, but rarely in well defined crystals: it is combined with a great variety of earthy substances, but its combination with metals are still more numerous. Its compounds contain from two to five substances, and even more.

Jasper. Jasper is not common, but is sometimes met with in the metallic veins, or those of quartz. Its varieties are brown, reddish, yellowish, and blackish.

Garnets. Garnets are very common in the rocks of white micaceous feldspar, but they are very small, being at most 2 millim. [0·78 of a line] in diameter. Those that are found in the gneiss are larger, but less distinctly formed. They have not many varieties either of figure or colour.

Feldspar. The veins produce some handsome varieties of feldspar, which is tolerably abundant in some parts of the mountain, and frequently in well defined crystals.

Tourmalines. Tourmalines are found in a rock of white micaceous feldspar, in tracing a vein of sulphuret of lead at Lafare. They are in very distinct crystals several centimetres long, and about 1 cent. [3·9 lines] in diameter. They frequently occur of a cylindroid figure in the heart of the solid rock.

Violet schoerl. A few small veins of axinite associated with epidote are found in a hornblende rock at the foot of the mountain near the cascade of Batou.

Green schoerl, or pistacite. Epidote is very abundant in the veins, sometimes well crystallized, and of a fine deep green, but very brittle. More frequently it is in mass, and sometimes constitutes whole rocks. Most commonly however it lines the sides of clefts in hornblende quartz rocks.

Hornblende & strahlstein. Amphibole and actinote, which the analyses of Mr. Laugier have united into one species, are abundant, and form masses of considerable bulk. At the bottom of the mountain, under the vein of Lafare, where a considerable portion of cliff fell down some years ago, there is a rock of white or gray feldspar, containing some beautiful needles of amphibole of a blackish green. This rock exhibits one

An ore of titanium,

and of magnetic iron.

of the most beautiful varieties of syenite granitello. Two associations peculiar to the amphibole of Chalanches are, 1, a calcareo-siliceous titanite in crystals of a lemon yellow, which form a pleasing contrast with the dark green of the amphibole in laminar masses: 2, a magnetic oxidule of iron in indeterminate crystals of a black metallic brown with a laminar fracture.

Chrysolites.

Some veins of arseniated cobalt ore very rich in silver, and even frequently presenting this metal in its native state, contain a mixture of small greenish crystals, which might be taken at first sight for granulous epidote, but appear to me to be peridot. I could not observe any distinct figure in them: they are disseminated in small irregular grains amid arseniated or oxided cobalt; frequently they are with sulphuretted silver; and sometimes they are covered with native silver. This mixture is one of the least common at Chalanches, but it is one of the most remarkable for the variety of its constituent substances, and its richness in silver amounts to 18 or 20 hect. of metal in a myriagr. of ore [18 or 20 per cent].

Mica.

Mica is very abundant in the rocks of Chalanches. It is in hexaedral laminæ frequently very well defined; and in colour white, yellow, gray, or blackish. The celebrity of the mines of Chalanches, and the appearance of the great quantity of silvery white mica in its cliffs and declivities, have led many persons but little versed in mineralogy to suppose, that the whole mountain was formed of silver.

Asbestus.

Asbestus is very common in the veins at Chalanches. It is found in different states, and modified by various mixtures. Frequently it is very rich in silver, and thrown into the furnace with the ore.

Amianthoid.

The amianthoid of Chalanches is in fine, slender, silky needles, sometimes stiff and elastic, of a silky green colour. It is frequently tinged by oxide of iron, or of manganese.

Chlorite.

Chlorite talc is very abundant in the veins, both in compact masses and pulverulent; and in each state it frequently contains native silver.

3d class.

Sulphur.

In the third class, combustible substances not metallic, there are only sulphur and anthracite. Sulphur is very common as a mineralizer, and has sometimes occurred native in a whitish yellow powder. The anthracite is found foliated, scoriform, striated, and earthy; and is frequently traversed by small veins of quartz, exhibiting very pretty crystallizations, that form a striking contrast with it.

Anthracite.

4th class.

We now come to the fourth class, that of metallic substances.

Gold.

Gold has been reported to have been found at Chalanches native, or alloyed with native silver, but this is erroneous. Mr. Schreiber however, in his analysis of the copper pyrites of the vein of St. Lewis found it to be auriferous.

Native silver.

Native silver is very abundant in the veins at Chalanches. It has never been found crystallized, but in various forms, as branchy, filamentous, capillary, lamellar, granulous, amorphous, pulverulent, and earthy. These eight varieties are mixed with a number of different substances, and in particular with carbonate of lime, quartz, epidote, chlorite, peridot, copper, arsenical nickel, carbonated nickel, earthy oxide of cobalt, arseniated cobalt, oxide of iron, oxide of manganese, oxide of mercury, lead, antimony, &c. The native silver is found sometimes mixed with only one of these substances, at others with two or three of them, but more frequently with all of them together.

Antimonial silver.

Antimonial silver occurs very rarely.

Sulphuretted silver.

Sulphuretted silver is very rare in well defined crystals: but it is found lamellar, filamentous, amorphous, and scoriform, disseminated in the same gangues as the native silver, and with the same mixtures.

Horn silver.

Muriate of silver has never been found crystallized but once, when it was in a perfect cube, and the first time it had been noticed. It has since occurred frequently in a violet powder on the surface of native silver, or argentiferous cobalt ores; but it never forms more than a very thin coat on it.

Sulphuret of silver and antimony.

Sulphuret of silver and antimony is found occasionally in small amorphous masses. Carbonate of lime tinged with oxides of iron and manganese are its most common gangues.

Argentiferous ores.

Beside these five states silver has frequently been found in sulphuret of lead, gray copper ore, arsenical nickel, &c.;

but it is seldom if ever visible in these ores, and only to be detected by an assay.

Mercury. Mercury has been found here native but once. It was in a calcareous gangue, coloured by sulphuret of mercury and oxide of manganese.

Cinnabar. Sulphuret of mercury is very common among the argentiferous ores. Frequently it is so concealed by the oxides of iron, manganese, and cobalt, that it is only to be found by assaying. It occurs likewise with the sulphurets of zinc and lead, and some others.

Galena. Sulphuret of lead is not abundant, but, when it does occur, it is moſt commonly very rich in silver. It is frequently iridescent.

Carbonate of lead. Carbonate of lead is very rare. It is found in the cavities of sulphuret of lead, quartz, and some argentiferous ores. It is seldom crystallized; and when it is, it has the trihexaedral form. Most commonly it is acicular, and sometimes earthy.

Arseniated lead. Arseniated lead is found pulverulent or earthy in cavities of sulpheuret of lead. It is but rare.

Phosphated lead. Phosphated lead sometimes occurs in small, fine, distinct needles of a yellowish green colour, on the surface of sulphuretted lead that is full of hollows.

Molybdate of lead. Molybdate of lead was found by Schreiber at the foot of the mountain, near the cascade of Batou, after a great landslip. It is in a fine greenish schistous hornblende rock mixed with feldspar, intersected by small veins of green epidote, frequently crystallized in fine transparent needles. Among these needles the molybdate of lead is found.

Gray copper ore. Gray copper ore is very abundant, but never crystallized. It is commonly very rich in silver; and among the combinations for which it appears to have a preference is that of silky asbestus.

Copper pyrites. Pyritous copper sometimes constitutes veins of itself. A few of these were auriferous.

Carbonate of copper. Green carbonate of copper is frequent in the argentiferous mixtures. It is commonly superficial, or as a colouring principle.

Nickel. Some of the veins have yielded fine and rich specimens of nickel, in which silver has frequently formed more than one sixth of the mass.

Arsenical nickel.

Arsenical nickel occurs very pure in nodules, though rarely. It is more frequently mixed with cobalt, iron, and silver. Frequently it contains silver enough to be worth working as an ore. This is a combination peculiar to Chalanches. It is in irregular nodules, of different sizes, capable of being cut and polished.

Oxide of nickel.

Oxide of nickel occurs frequently in fine specimens commonly covering arsenical nickel. It is white, or of a green more or less deep, and always pulverulent.

Magnetic oxide of iron.

Magnetic oxidulated iron is frequent in the hornblende rocks. It is in crystals of an indeterminate form, of a deep black colour, and with somewhat of a metallic lustre.

Specular iron ore.

Oligist iron is very abundant in some veins, is frequently found in the strata of gneiss, and is common in those of quartz, and carbonate of lime.

Pyrites.

Sulphuret of iron is not abundant, but sometimes occurs in the veins, and in the rocks. Occasionally it is without its yellow colour and metallic lustre, having acquired a dull and earthy aspect from decomposition.

Oxide of iron.

Oxided iron is the most common gangue of the silver ore. Sometimes the silver can be discerned in it by the naked eye; but it is more commonly concealed by the iron, which is more or less coloured and mixed with foreign matters.

Iron spar.

The sparry iron ore has been mentioned already under the head of carbonate of lime.

Manganese.

Manganese is very abundant in the veins, but hitherto it has been found only in the state of oxide. It accompanies the richest silver ores.

Zinc.

Oxide of zinc occurs, though rarely. Sulphuretted zinc is rather more common. Neither of them is crystallized.

Arsenical cobalt.

Arsenical cobalt is scarce, never forms veins alone, and commonly is destitute of silver.

Gray cobalt.

Gray cobalt is rather more common, but seldom pure. It is frequently mixed with arsenical nickel, and sometimes with silver.

Black oxided cobalt.

Black oxided cobalt is very abundant, sometimes pure, but commonly mixed with oxides of iron and manganese, sulphuret of mercury, and often silver.

Arseniated cobalt.

Arseniated cobalt is met with in some veins, but never forms veins of itself. It pretty frequently indicates the proximity of veins of argentiferous oxide of iron. The argentiferous earthy arseniate of cobalt at Allemont contains from 1, 2, or 3 ten thousandth parts of silver to ten or twelve per cent.

Native antimony.

Native antimony is very rare at Chalanches. It is in large or small shining metallic scales confusedly arranged, and forming masses capable of being divided parallel to the faces both of a regular octaedron and a rhomboidal dodecaedron. Sometimes it is in solid compact nodules capable of being cut and polished. The presence of arsenic, though in very small quantity, changes entirely the texture of the antimony. In this state it forms a kind of scales, the surface of which is frequently undulated.

Sulphuret of antimony.

Sulphuretted antimony is found in the same situations as the preceding, but it is infinitely more scarce. It is prismatic or laminar, but more commonly amorphous.

Oxide of antimony.

Oxided antimony occurs in rectangular crystalline laminæ in the interior of the native antimony; but more frequently pulverulent, white, and earthy, on the surface of the nodules with large facets.

Hidrosulphuret of antimony.

Hidrosulphuretted antimony is still more rare than the preceding species, but it is found with the oxide in the superficial cavities of the native antimony with large facets.

Arsenic.

Arsenic is never found native, but is sometimes combined with the nickel, cobalt, or antimony.

Titanium.

The greenish or blackish amphibolic rocks frequently contain lemon-coloured crystals, which have been found to be a siliceo-calcareous ore of titanium.

VI.

Effects of Gravity on the Balance of a Watch compared with those on the Pendulum of a Clock. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Effect of gravity on the ba-

(*a*) I Have been often led to compare the balance of the watch with the pendulum of the clock. The functions of both being to regulate the motion of the respective instruments, similar methods have been adopted in each for compensating the effects of heat and cold. But the difference of latitude which has been so fully recognized as affecting the pendulum has not yet, as far as I know, been considered to have any influence on the balance.

lance hitherto neglected,

Gravity should accelerate the motion of a watch.

(*b*) The time of the fall of the pendulum is in the inverse subduplicate ratio of the force of gravity. In the watch, on the other hand, the time of the vibration of the pendulum spring is in the direct subduplicate ratio of the weight of the balance. It appears to me, therefore, that any approach toward the equator, by diminishing the gravity of the balance, must tend to accelerate the motion of the watch in the same ratio by which it retards that of the clock.

Difference of rate at the pole & the equator.

(*c*) Suppose the diminution of gravity at the equator $\frac{1}{229}$ = 0·004367. Let the weight of a balance vibrating mean time at the pole be denominated 1. Being removed to the equator, its weight will be $1 - 0·004367 = 0·995633$, and the time of one vibration at the pole, to the time of one vibration at the equator, will be as $\sqrt{1}$ to $\sqrt{0·995633}$, or as 1 to 0·997814; consequently the number of vibrations in one day at the equator will be to those at the pole as $\frac{1}{0·997814} = 1·002191$; which reduced to time gives 24^h 3′ 9·3″. That is, the watch will go too fast 3′ 9·3″. If the diminution be taken at $\frac{1}{190}$, agreeing with a printed table of the variation of the pendulum to every fifth degree of latitude, then by the same process, the errour of the watch will be 3′ 48·3″ daily.

Experiments with the pendulum should differ from those with the balance.

(*d*) The errour here is so great, that in proceeding from this latitude to the line, it is not possible, that it could have escaped observation, with the almost numberless experiments that are now made with watches; but if the reasons for the variation of the balance be found just, these experiments will stand in opposition to those that have been made with the pendulum. With respect to the latter, they have been conducted by men of such eminence, that we should have no reason to doubt their accuracy, if they did not differ so widely from one another.

(*e*) Mr. Richer found by experiments, that a pendulum, which

Experiments with the pendulum do not agree.

which vibrated seconds at Paris, required to be shortened a line and a quarter at Cayenne, four degrees from the equator. Mr. Couplet the younger observed, that a pendulum, which swings seconds at Paris, must be shortened 2½ lines somewhere in Portugal, which is more than Sir Isaac Newton allowed from the pole to the equator. Messrs Picart and de la Hire found the length of the pendulum, which beats seconds, the same at Bayonne, at Paris, and at Uranibourg in Denmark. Mr. Cassini pretended to prove from experiments, that the polar diameter is the longest. In short, conclusions can hardly differ more widely.

(*f*) It may be thought, that a decrease of gravity will not have the same effect as lessening the weight of a balance. I suppose gravity constitutes weight. They decrease and vanish together. It is the inertia of the balance, that regulates the vibrations of the spring; but its inertia arises from its weight. They decrease and vanish together. I do not mean however to assert, that there is no variation of gravity in different latitudes. I only suspeet, that it is considerably less than the received opinion allots to it, and that it is probably irregular. The centrifugal force about the equator is a principle evidently tending to lessen the effect of gravity; but the internal structure of the Earth, of which we know nothing, may lessen or vary its effect.

Experiments with the pendulum and balance may be made within 150 miles.

(*g*) Those who have inclination and opportunity to make experiments or observations on this subject will have no occasion in the first instance to go any great distance. Two degrees about this latitude, according to the table, produce not less than 7½″ per day of errour in the pendulum; and if the balance be found as accurate a measure of the variation of gravity as the pendulum by operating the contrary way, there will arise a difference between them of 15″ per day; a quantity very discernible. Many watches are to be found, that do not vary in their daily rate so much as 7½″ for a considerable time, and clocks not nearly so much.

Portable pendulum rod in different pieces.

(*h*) A pendulum rod might be made more portable, by being in three pieces to join together with perfect accuracy, and consequently not subject to be bent, by which the length would be altered; and the ball being also fixed fast, and the time of the vibration ascertained in some latitude, it might be taken in pieces, and when reconstructed elsewhere the difference of time would show the length requisite more correctly than any other sort of measurement, and consequently the difference of gravity. Several little errours would be avoided by always using the same pendulum.

Vibrations of a balance do not depend on gravity.

(*i*) Mr. Cumming, in his Elements of Clock and Watch Work (notes to Article 410) says, "In the pendulum, gravity is the motive force; and in the balance the spring: the vis insita is the resistance in each; and the contrary: therefore, when the motive force is in each as the resistance, the velocities and times must be equal. Hence it also happens, that (*c. p.*) the balance measures the same time in all latitudes." And afterward: "Here by altering the weight, is strictly meant, altering the vis inertiæ; for the vibrations of a balance, whose centre of gravity coincides with its centre of motion, have not the least dependence on gravitation, otherwise it would alter its times in different latitudes as well as the pendulum."

(*k*) Here it appears to me, that Mr. Cumming considers the vis insita and vis inertiæ of matter, as powers wholly independent of its gravitation or weight. I suppose it will not be doubted, that a balance becomes heavier or lighter, as gravity is greater or less, and if hung by its spring, it will stretch the spring proportionally to the power of gravity: still he considers its vis insita to remain unaltered with its motive force, and consequently that it will measure the same time in all latitudes.

Vis inertiæ.

(*l*) I think it is generally allowed, that the vis inertiæ is proportional to the quantity of matter. We distinguish a greater or less quantity of matter by its weight, consequently its vis inertiæ will be proportional to its weight, for it is certain that any thing heavy has more resistance than a thing that is lighter.

Balances of different weights require springs of different strengths.

(*m*) Suppose a watch vibrating with a steel balance, and, without any other alteration, let it be taken off and replaced with a gold balance of equal magnitude in every way, which, for convenience we shall suppose to be just double in weight. Though in this case nothing is altered but the weight, the inertia is just doubled, for it will require another spring equally strong, added to the former, to give it the same velocity.

(*n*) Again

Enlarging a balance has a similar effect.

(*n*) Again, every thing remaining as before; suppose I beat out this gold balance till its diameter be just doubled, or, more accurately, till its diameter of percussion is just double: here the inertia is quadrupled, for now it will require eight springs equally strong with the first to make it vibrate in the same time, and generally, let D be the diameter of percussion, and W the weight, the power of any balance will always be as $D^2 \times W$, for the strength of the spring must be in this proportion to make it vibrate in the same time. If the number of vibrations are changed, that is more or less in the minute, the velocity in similar parts of each vibration will be as this number, and if it be denoted by N, the power of any balance will be as $\overline{N^2 \times D^2 \times W}$, the spring being in this proportion.

(*o*) Mr. Cumming goes on to say, that "the vibrations of a "balance, whose centre of gravity coincides with its centre "of motion, have not the least dependence on gravitation."

Surely this cannot mean, that balances of different weights, having their spring or motive force the same, will vibrate in equal time; but either this must be the case, or he must consider a variation in the power of gravity as not altering the weight of any thing.

Variations of timekeepers at sea.

(*p*) I have heard it remarked, that timekeepers do not go so well at sea as they are found to do on land. But as all considerable changes of place are made by sea, it is possible, that the influence of latitude on gravitation may be the unsuspected cause of some part at least of this deviation. If the principle I have suggested be not erroneous, it will be of more importance than ever, to ascertain the exact variation of this influence. But as I am very conscious, that my attainments in science are not such as to entitle me to be positive in a matter of this magnitude, however plain it may appear to my own mind, I am solicitous to be favoured with your judgment upon it, or that of any of your correspondents.

I am, Sir,

Your most obedient humble servant,

X.

ANNOTATION.

We assert, that the gravity of a body, is as its mass, and that its mass is as its inertia, or the power by which it tends to preserve its state as to motion. Admitting or assuming the inertia to be invariable, we deduce, that gravitation varies inversely as the square of the distance between gravitating bodies; and our observations on the relative situations and motions of the heavenly bodies show, that this is the case; or else, that, if the inertia be subject to variation, its changes are such as to produce, along with some correspondent opposite change in gravity, a result of the same kind, as was accounted for on the supposition that the inertia does not vary. But we seem to have no decisive facts to determine as to our original hypotheses, and therefore prefer the simplest. Though the author of the preceding letter appears to have confounded the terms gravity and inertia; yet the experimental speculation, to which his letter points, appears to be of sufficient interest to lead to meditations of some value, and to authorize its insertion in our Journal.

Inertia considered as invariable.

But decisive facts are wanting,

so that there is room for inquiry.

W. N.

VII.

An Account of a simple and economical Method of preparing an artificial Cheltenham Water highly impregnated with Carbonic Acid (fixed Air). By RICHARD GREENE, *Esq. of Cork, A. B. Trin. Col. Dub., M. D., and President of the Royal Medical Society Edinburgh.*

To Mr. NICHOLSON.

SIR,

IF you think the following communication merits a place in your valuable Journal, it is perfectly at your service, and by its insertion you will confer an obligation on

Cheltenham water of high repute.

Your most obedient humble servant,

Edinburgh, Dec. 27, 1808. R. G.

The purgative and chalybeate waters of Cheltenham have long and deservedly been celebrated, in the cure of many obstinate and alarming diseases. The chief obstacles to their more general employment seem to be, the impossibility of procuring the natural water in a proper state except at the spring; or the scarcity and exorbitant price of the aërated artificial imitation, which too is very seldom to be met with properly prepared.

The carbonic acid of essential importance in it.

The simple solution of the saline matter of these waters is often prescribed with the most happy effects. This, however, entirely wants one very active and important ingredient of the natural water, the carbonic acid; by the lively pungency of which, when in sufficient quantity, the offensive taste of the salts is concealed, and a nauseous medicine is converted into an agreeable beverage. This is only one, and perhaps the least of the advantages of the carbonic acid. By virtue of it, and the iron which it holds in solution, the use of this water may be persevered in for a long time without inconvenience: for during its employment the appetite will be improved, and the digestive organs strengthened: and the greater quantity of this acid the water can be made to contain, the less offensive will it be to the palate, and the less it will be liable to be rejected by the stomach, while the whole system, sympathizing with the tonic effects it produces on that organ, will be the more speedily invigorated.

The more of this it contains the better.

Its chief uses.

It would be out of place here, to enter largely into the medicinal properties of this water*, I shall just enumerate the principal affections of the system, in which it has been found particularly useful. Those, whose biliary organs a long residence in a warm climate has impaired, seldom fail to receive much benefit from a course of Cheltenham water, and its use may be continued even under circumstances of great debility: and will be eminently serviceable if any symptoms of dropsy or anasarca threaten, as so often happens in affections of the liver. In glandular obstructions its use has often been attended with success. In all cases where the secretion of bile is vitiated, or irregular; and in jaundice from resistance to a free discharge from the gall bladder, attended with sense of heat and distention after eating, this water will be employed with particular advantage. For removing a sense of fulness about the head in the plethoric, and for carrying off the effects of any excess in eating or drinking, no medicine seems more proper than this laxative diluent. For habitual constipation of the bowels, as the stimulus of this water is so slight that its frequent repetition cannot be productive of injury, it may be considered the safest as certainly it is the most pleasant remedy.

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* See Dr. Saunders's Treatise on Mineral Waters, 1805.

An artificial water, equal if not superior to the natural, easily prepared.

From the many valuable properties of this mineral water, I am firmly persuaded, that it would be far more extensively employed than it is at present, if it were possible to convey the natural water to a distance unimpaired, or to procure at a reasonable price an imitation of it, that could be relied upon. I therefore trust, it may not be altogether useless, to lay before the public a simple and economical method, by which any person may in a few minutes prepare a water eminently possessing the virtues of the natural spring, as composed of the same ingredients, but more palatable, as containing a larger quantity of the carbonic acid, which also increases its tonic powers.

Ingredients of the natural water.

The natural Cheltenham water owes its virtues principally to the sulphates of soda and magnesia *(vitriolated natron, and vitriolated magnesia)* which are its purgative constituents. It contains also about one eighth of its bulk of carbonic acid, which holds in solution nearly five grains of iron in each gallon of the water. The sulphate and carbonate of lime, which may also be detected in it, are not likely in any respect to influence its medicinal properties.

Impregnation of water with carbonic acid.

The carbonic acid employed in preparing artificial mineral waters is obtained by decomposing the natural carbonates of lime *(marble or chalk)*, by means of the sulphuric *(vitriolic)* acid, which disengages the carbonic, in its elastic form, to be afterward forced into the water by mechanical pressure, and apparatus which has not yet been disclosed to the public.

Carbonates of the bases in Cheltenham water give out this acid when employed to compose the neutral salts in it.

The carbonates of soda and magnesia (common soda or prepared natron, and magnesia alba) contain a large proportion of condensed or combined carbonic acid essential to their constitution; the former about one sixth, and the latter nearly half its weight. They likewise are both decomposed by a proper quantity of the sulphuric acid, which, uniting to their bases the soda and magnesia, forms the sulphates of these bases, or the salts of Cheltenham water; the carbonic acid escaping with effervescence in its aëriform state.

It

and this may be retained in the water

It occurred to me, that if by any means this gas could be confined at its first disengagement, and a sufficient quantity of water present, it would be absorbed by that very water in which it would be disengaged, and serve the same purpose as when afterward driven into it by the force of a condensing apparatus. I made many experiments to this effect, with different proportions, and with different quantities of the ingredients, and with the most satisfactory results: and found, that the entire process could be safely and conveniently conducted, in a pint, or half pint bottle of common green glass. Having calculated what quantity of the carbonates of soda and magnesia *(properly proportioned)* would yield on decomposition a volume of carbonic acid, that might without danger be confined in a half pint bottle; I found the quantity of dilute sulphuric acid necessary to decompose it, and measured it out in a graduated glass. I then put the carbonates into the bottle, together with one grain of the sulphate of iron *(green vitriol)*, and nearly filled it with cold water; just leaving room to add the sulphuric acid, which I next put in, and instantly closed the bottle, securing the cork with a string. By a little agitation, the salts were soon dissolved, and the liquor became transparent: and on removing the cork, I had the satisfaction to find the process had succeeded equal to my most sanguine expectations: for the liquor was so highly impregnated with the gas, as to be scarcely distinguishable in taste from the best prepared soda water, and it flowed with ebullition from the bottle, the instant the string was untied, driving out the cork with considerable violence. The quantities here employed were too great for general use, being nearly one half greater than the proportions, which farther experience has enabled me to recommend: however, they show, that the strength of the water may be much increased without bursting the bottles.

in a common pint bottle. Experiment proving this.

Farther particulars of the process, and cautions to be observed in conducting it.

Process for making Cheltenham water.

The carbonate of soda employed was the common soda of commerce, that prepared from the muriate of soda *(sea salt)* by the patent process, which neither deliquesced nor effloresced by exposure to the air; and by a very delicate test gave no indication of the presence of lead, which has been suspected to be contained in soda manufactured in this way. The quantity I would recommend is two scruples, or forty grains troy, for the half pint of water. The carbonate of magnesia I made use of I prepared myself from sea water, and therefore I could depend upon its purity. The quantity for the same dose of water is one scruple, or twenty grains: the half of the quantity of the soda. The sulphuric acid I employ is the dilute acid of the Edinburgh College; of the specific gravity of 1·0735, and was formed, by adding one part (by weight) of the strong acid of commerce, of the specific gravity of 1·852, to eight parts of water. The quantity of this acid I use is 250 grains by measure, or a quantity equal in bulk to 250 grains of water. In this proportion, there is a slight deficiency of sulphuric acid, which should always be observed; for the excess of carbonic acid will be able to dissolve any carbonate of magnesia that may remain, and any excess of sulphuric acid gives the water a harsh disagreeable taste, and might injure the teeth, or affect the bowels, if irritable. The volume of carbonic acid gas disengaged by these quantities is about 35 cubic inches, and in a half pint bottle is equal to the active pressure of an additional atmosphere and a half on the sides of the bottle, or to two atmospheres and a half upon the surface of the water. The sulphuric acid is best to be diluted, as by this any errour in measuring will be diminished, and by dilution it throws down a sediment, which often contains a portion of lead.

Indication of deficiency of soda or of acid.

As the carbonate of soda is unsteady in the quantity of water it contains, being affected by the dryness or humidity of the atmosphere, and the sulphuric acid of commerce is not always of the same strength, it may happen, that the quantity of the acid I have here specified shall not exactly answer: if so, any excess will manifest itself by an acid taste in the water after it has stood some time exposed to the air in an open vessel; and any great deficiency of acid, by the whole of the magnesia not being dissolved. In this case the quantity of acid must be increased or diminished as indicated. But I would advise every person, who would wish to give this process a fair trial, to appreciate by experiment

The proper proportions best ascertained by an easy experiment.

riment the quantity of acid required for the soda and magnesia he may employ; as it is very simple, and easily performed. Weigh out the proper quantity of the carbonates, and put them into a common wine or ale glass, with about an ounce of water; add gradually the acid to them, constantly stirring them until they are dissolved, and all effervescence has ceased; taking care, that no excess be employed, which may be known by the acid taste, or by turning to a red the natural blue vegetable colours, (as infusion of litmus, or of red cabbage leaves in hot water); note exactly the quantity of acid that was used, and make a measure containing ten drops less than that quantity, which is easily done by marking a slender ounce phial or glass tube, by means of a file, or a diamond. If this is found to answer well, a good parcel of the same ingredients as those of the experiment should be procured and kept in close vessels.

Farther precautions.

The corks should be previously fitted to the bottles in which the artificial water is to be prepared, that no time may be lost after putting in the acid; and the bottles should always be kept on their sides, that the liquor may cover the corks, and thus prevent the escape of the gas. The magnesia should be in fine powder, that it may dissolve more readily: the soda is best not to be so, as by its slow solution, the magnesia will meet the acid in a more active state, and there is no danger, that the soda will not dissolve readily enough. By minding these precautions, a bottle of the water may be made ready for use in less than a minute: but it will keep very well, only observing to shake the bottle before opening it, as the iron precipitates in part.

Strength of the water may be varied.

General proportions and price.

The strength of the water may be varied at pleasure, by altering the quantities of the salts with respect to the water, but minding the relative proportion of the acid to the carbonates. For a pint bottle the quantities are: carbonate of soda four scruples, or eighty grains; carbonate of magnesia two scruples, or forty grains; sulphuret of iron two grains. This is a full dose for an adult, and more than should be taken for a constancy, during a course of this water. The expense of preparing a pint bottle is about one penny; the strong sulphuric acid or oil of vitriol, is only eight pence per pound, and the soda the same price, or nine pence, by the single pound.

VIII.

VIII.

Second Letter on the Advantages of Coal Gas Lights. By Mr. B. COOK.

To Mr. NICHOLSON.

SIR,

On the advantages of gas light.

IN my last letter I promised you a few plain drawings, but I have been out of Town nearly three months since I wrote to you, which has hindered me from sending them. I am desirous, however, if possible, to hand you the few following remarks, in addition to my last letter, in time for your Journal.

Where no soldering is required.

First, with respect to oil and cotton, the man who does not use these articles, may unthinkingly strike that out of the statement, and then there appears but little or no advantage from the adoption of the use of the gas lights. But he must not do this: for, if he do not use these articles, a large portion of the expense in the execution of the apparatus is saved, and one lamp will consume as much gas as nearly twelve lights, or candles. If this is duly considered, the saving is the same, or nearly so; for less gas will be wanting to be made, less coal consumed, and the man or boy, that makes the gas, will perhaps be required to make it only two or three times a week, instead of every day, which will be the case, if the gasometer is small and lamps used. Instead therefore of employing a man a part of his time at five shillings per week, a boy will be employed to do it at perhaps two shillings per week or less. Indeed in most manufactories a boy or an old man is kept to job, or go on errands; and he will be able to do this and all his other jobs besides; so that in most manufactories this man, which forms a very great portion of the expense, may be entirely struck out. Besides, the industrious man, who works in his own shop, may do it himself, as the trouble is little, only putting a fire under the retort, and filling the retort with coal; when it will require no farther attention, except to see now and then that the fire is kept up.

Secondly,

Leaden pipes preferable to old gun barrels.

Secondly, I recommend the use of old gun barrels f
pipes: but since I wrote my former letter, I have been i
duced in a great measure to change my mind; for, havin
been employed in putting up the apparatus for some pe
sons in this place, I find the promised advantage is not
great as it first appears. Being in short lengths, they r
quire many joints, or soldering places, and if they be n
properly sound and air-tight, the gas escapes, a disagree
ble smell is produced, your gas is wasted, and the process
disliked. Now the patent lead pipe may be had in lengt
of fifteen or twenty feet, so that even in extensive manufa
tories but few joints will be wanting. The pipes likewi
can be made three quarters of an inch or one inch in th
bore or hole, which is an advantage; you can bend the
round any angles; and there will not be so great a proba
bility of leakage, when there are but few joints. I thin
therefore, although the lead pipes will be rather more ex
pensive, yet when all their advantages are considered, it wi
be better to adopt them, than the other. Besides, whe
the pipes are done with, and a man gives over trade, or whe
he removes his manufactory, he can very easily remove thes
and put them up again, or sell them for a great part of the
original cost for old lead. There is also another saving in the
workman's putting up the apparatus, as he puts up the lead
pipes in half the time he would the old gun barrels.

Perhaps therefore we may make the following statement,
for in fact it will be in general the true statement, where
only lights are wanting for twenty weeks in the year.

Corrected statement.

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Coal for twenty weeks, at 1*s*. 3*d*	1	5	0	Twenty weeks, at 1*s*. 8*d*.	18	0	0
Interest on £20 or 25	1	5	0	Coak	0	12	6
Saved	16	2	6		£18	12	6
	£18	12	6				

Here I have only allowed the half of my former statement
for the expense of coal, supposing no soldering is done, for in

that case not half will be used, and twice or three times a week will be quite sufficient for making the gas.

I have also left out entirely the expenses of a man, as this will be seen to be quite unnecessary, so little time will be required to make and attend it, if only used for lights.

The apparatus too in this case will not cost more than twenty or twenty-five pounds, so that I have allowed only one pound five shillings for interest. And although I have reckoned only twenty candles to be used in this statement, and have calculated therefore on the saving of twenty; yet twenty-five, or even more may be lighted, and at a very little additional expense, as it can only be in a little more coal being consumed; so that, if I were to reckon, that twenty-five or thirty candles were used, you see what a flattering advantage would appear. But I am not desirous of painting the picture with glaring colours, the saving in this statement is sufficient to satisfy every man, who is interested in it, of the great utility of the thing.

Its disagreeable smell not noxious,

There is one thing more I would advance in its favour. It has been objected to by several, on account of its disagreeable smell if any of the gas escape, or if the gas have not been properly purified. Now, Sir, the very thing they object to is an advantage. I would ask any medical man, whether the smell of burnt tar be injurious to the constitution. Why burn pitch and tar, to destroy contagion, if they have not the power of doing it? The gas will be found, instead of destroying health, to promote it, even in the last stages of consumption. It is a fact, the smoak of the coal fires in all large manufacturing towns, instead of injuring the health of the inhabitants, is good for it; for the contagious diseases are hardly ever known to appear, much less to increase in them. And how can we account for the prevention of these diseases, and for their not making sad havoc and devastation, in places, to all appearance so well calculated for their seat; crowded manufactories, crowded streets, filthy drunken workmen, the whole town at times quite enveloped with smoke; if we do not attribute it to its real and true cause, the coal smoke?

but conducive to health.

The introduction of the gas into manufactories will be found still more useful and beneficial, as it will destroy the inconvenience so often complained of in crowded shops and manufactories, where so many people are forced to breathe the same air over and over again, and which really is injurious to a very great degree. I say it will remedy this by purifying the air. I have no doubt it will completely hinder any thing like fever from spreading in those shops and manufactories, if it should be accidentally introduced. In fact it will be found to purge and purify the air, and to promote health, instead of injuring it.

I am, Sir,
Your humble servant,
B. COOK.

Caroline-street, Birmingham, Dec. 27th, 1808.

IX.

On the Superiority of Platina for making the Pendulum Spring of Watches. By Mr. James Scott.

To Mr. NICHOLSON.

SIR,

Balance spring capable of improvement.

AS your valuable Journal is universally esteemed a source of the best information and most general utility to the mechanical as well as the philosophical world, I beg leave to trouble you to insert a few remarks on the subject of the pendulum spring, as it is a part of watch work I have often considered capable of the greatest improvement.

Platina the best material,

I have been long convinced of the superior advantages of platina over any other metal of which this instrument has hitherto been made, but I have not until lately been able to procure any of it in wire fit for that purpose. I find it by repeated experiments to possess great superiority on account of its inperceptible expansion; and, what is worthy of remark, the platina procured for this purpose should not be consolidated by arsenic, as it is by that means liable to expansion. I also find it, when properly drawn, to possess sufficient elasticity for any extent of vibration; it coils extremely well; and, if placed when coiled on the surface of a flat piece of metal, making one end of the spring fast, and marking exactly the other extremity, the slightest expansion

but arsenic must not be employed in consolidating it.

is not visible when applied to heat: so that if these springs are judiciously made of this metal, I am convinced they will turn out a general benefit to the public, which is the principal object of, Sir,

Your much obliged and obedient servant,

39, *Grafton-street, Dublin,* JAMES SCOTT.
June 20*th*, 1808.

P. S. Having for a length of time made use of this metal in my compensation curbs, I consider it as very superior to steel for every instrument of this kind.

Superior to steel for compensation curbs.

N. B. This paper would have appeared much earlier, had it not been accidentally mislaid.

X.

On the Construction of Galvanic Batteries. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

EVERY lover of science in the country must have felt the great advantage of a philosophical Journal, in which he never fails to meet with a judicious selection of the most important discoveries, and whence he may hope to derive information, that would be vainly sought after in any regular treatise.

Queries respecting the construction of galvanic batteries.

Some friends, who have united their efforts to follow Professor Davy in his grand experiments on the decomposition of the alkalis, would feel highly gratified by replies to the following questions.

What is the smallest number of Galvanic combinations, and the smallest surface of plates, that is sufficient to decompose the fixed alkalis? And, what is the best solution for charging a battery, so as to produce the greatest power?

We have seen, that batteries formed of the common rolled zinc do not act so powerfully as those, which have been made with zinc plates that were cast. It is said, that zinc is commonly alloyed, to make it roll the easier; but it is certain, that it may be rolled much thiner than is required for this purpose, in its pure state*.

I am, Sir,
Your obedient servant,

Jan. 2*d*, 1809. G. K. M.

REPLY.

Having taken the liberty of troubling Mr. Davy with a line on the subject here stated, I was favoured with his answer, from which I extract the following.

Battery capable of decomposing potash.

" In my early experiments upon potassium, I often procured it by means of a battery of one thousand pairs of plates of copper and zinc of six inches square, charged with a solution of concentrated nitrous acid in about forty parts of water. This is the lowest power that I employed: but as some of the plates had been much corroded by former processes, I should conceive, that a combination of eighty would be sufficient, provided the whole arrangement was perfect.

Less power sufficient for the earths and ammonia.

" The decomposition of the alkaline earths and ammonia by amalgamation or combination of their bases may be accomplished by a much weaker combination, fifty plates of six or four inches square being adequate to produce sensible results.

Potassium may be procured without electricity.

" The potassium, which I have used in various analytical enquiries lately carried on, has been all procured by chemical means, without the application of electricity.

" Potash may be decomposed by different processes, some of which are described in a paper, which I am now reading before the Royal Society; but the best method is that, which we owe to the ingenious researches of Messrs. Gay Lussac and Thenard, and which is the first of this kind, by mere chemical attraction, made known.

Chemical decomposition of potash.

" When melted potash is slowly brought into contact with iron turnings, or filings, heated to whiteness, hidrogen gas is evolved, holding potassium in solution: and if one part of the iron tube, or gun barrel, in which the experiment is made, be preserved cool, the metal is deposited in this part, being precipitated from the hidrogen gas by cooling.

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* And at the common temperature. E.

" The

Near 100 grains in a mass procured by one operation.

"The potassium is never procured quite so pure in this manner, as by electricity; but it is fit for analytical purposes, and I have obtained it with so little alloy, as to possess a specific gravity considerably below 8, water being 10. I have now by me a compact mass produced in an operation which weighs nearly 100 grains."

XI.

Account of an economical Method of Evaporating the Water of Brinesprings, employed at the Salt Works of Moûtiers, in the Department of Mont-Blanc. By Mr. H. LELIVEC, *Engineer of Mines, &c. for the Departments of Mont-Blanc and the Leman Lake*[*].

Brinesprings at Moûtiers.

THE richest spring at Moûtiers is constantly at the temperature of 30° R. [99·5° F.], and when cooled down to 10° [54·5°] indicates on the areometer 1·83°[†]. The poorest raises the thermometer to 25° [88° F.] only, and indicates 1·5° of saltness. The water is conveyed by troughs to a large reservoir, where it is left to settle; and thence it passes through other troughs to graduation houses about 1100 yards lower down. In its course it gives out bubbles of carbonic acid gas, and deposits a reddish sediment, which is at first oxide of iron, then a mixture of this with carbonate of lime, and at length almost wholly calcareous carbonate. It passes through four graduation houses in succession, and comes out of the last at the strength of 18°, and sometimes more. It is then boiled for about six and twenty hours, or till the salt begins to crystallize, keeping the boilers constantly full; a foulness, that rises, is scummed off; and the sulphate of lime it contains is precipitated.

Method of procuring the salt from them.

The sulphate of lime being raked out, in winter the evaporation is continued, with a slow fire, till the whole of the salt is deposited: but in summer a different process is followed, by which all the fuel consumed in the last stage of the

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[*] Journal des Mines, No. 120.

[†] The areometer commonly employed in the French salt works is divided into 20 degrees, beginning at distilled water, and ending at water saturated with salt.

New method of crystallizing the salt without fire.

the process is saved. When the solution is brought to the point of saturation, it is conveyed to a reservoir, whence it is raised by a chain-pump to a trough at the top of a wooden building, and extended its whole length. From this trough it runs into a series of very narrow troughs at right angles to it, and about two yards long. To each of these are twenty-five double or endless ropes, 6 millim. [2·4 lines] in diameter, 13 cent. [5 inches] from each other, and fixed 8 met. [26 feet] below. The saline water, flowing constantly out at notches cut in the sides of the troughs, trickles down the ropes, round which it forms a very thin coat, displaying a considerable surface to the solvent power of the air. As the water evaporates, the salt is deposited on the ropes. The water that flows down runs into the reservoir, and is pumped up again repeatedly, till it is exhausted, when it is suffered to run into the basin, that contains the mother-water.

The water of a fresh boiling is treated in the same manner, and thus seventeen boilings are raised in succession, forming one making, which occupies forty or five and forty days. At the expiration of this time the ropes are covered with a cylindrical coat of salt 7 or 8 cent. [2·75 or 3·15 inches] diameter, which is broken by a particular instrument for the purpose. As this process can be executed only in summer, seldom more than two makings take place in a year.

Quantity of salt made.

Every boiling, before it reaches this building deposits 100 myriagr. [2205 lbs] of salt in the boilers; and 650 myr. [14332 lbs.] are collected from the ropes, making in all 750 myr. [16537 lbs.] This process therefore does not yield quite so much salt, as the product of the evaporation of a similar quantity of water by two boilings would be 786 myr. [17332 lbs.]; but then there is a considerable saving of time and labour, as well as of fuel, and the salt obtained is more pure.

The process has been followed 20 years.

This process, equally ingenious and economical, invented by Mr. Dubutet, has been employed with success ever since the year 1788. It has not yet been adopted in any other salt works. It would be particularly advantageous in hot and dry climates.

It may be used

Mr. Roche has enhanced the utility of this building, by employing it as a graduation house during the eight months in which it is not used for crystallization. He has found, that, all other circumstances being the same, the evaporation goes on nearly twice as fast in it as in the common graduation house with faggots. It is necessary however, that the brine should be of the strength of 4° or 5° before it is brought thither, otherwise the ropes would speedily rot.

as a graduation house, and is much superior to the common one.

This building with ropes, the only one in existence, is 90 met. [295 feet] long, 17 met. [55¾ feet] of which are taken up by the pillars and the machine. It is divided into six arches, by party walls covered with boards; and each arch contains 40 troughs, and consequently 2000 single ropes 8·3 met. [27·2 feet] long; so that there are in all 12000 ropes, making a total length of 99600 met. [326546 feet]. Little expense is required for repairs, as three fourths of the ropes last seventeen or eighteen years from the time they are put up.

Dimensions of the building.

XII.

Eclipses of the Satellites of Jupiter, observed by JOHN GOLDINGHAM, *Esq. F. R. S., and under his Superintendance, at Madras, in the East Indies*[*].

BETWEEN the beginning of the year 1794 and the end of the year 1802 Mr. Goldingham had 151 observations of eclipses of the first satellite of Jupiter, and 105 of the second satellite, either at their immersion, emersion, or both. Of these he has given tables, noting the day, whether immersion or emersion, apparent and mean time of observation, time by the Ephemeris, longitude of Madras by the tables, and circumstances of weather, &c. As the judicious remarks prefixed to these tables may be of use to future observers, it will not be unacceptable to several of our readers to find them here. They are as follows.

Eclipses of Jupiter's satellites.

The eclipses of the satellites of Jupiter were observed with achromatic telescopes, by Dollond, of three feet and half focal length, and magnifying power between 70 and 80;

Telescopes.

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[*] Phil. Trans. for 1808, p. 322.

so; having been constructed more immediately for this purpose, for which they were exceedingly well calculated in all respects.

Clock.

An astronomical clock, with gridiron pendulum, and dead beat, regulated by transits of the sun and stars, was used for the time; which was deduced from the transit of the sun nearest the eclipse, and verified by the one immediately preceding or following.

Circumstances noted.

The circumstances under which the eclipses were observed are noted; from these may be inferred, how far the results are to be depended upon: those observed with the "air clear and the planet high" are the most satisfactory and valuable, nothing to the contrary being afterward expressed.

Longitude of Madras.

The longitude of the place, by numerous observations of various descriptions, is 5h : 21′ : 14″, or 80° : 18′ : 30″ east of Greenwich: by comparing this with the numbers in the last column, the difference of the tables will be obtained.

The greater number of these eclipses were not visible at Greenwich, but have been found very useful, when correspondent observations have been taken in India.

General remarks.

Persons not much in the habit of observing these eclipses, but desirous of obtaining as much correct information from their observations as possible, may find the following general remarks of use.

Correspondent observations necessary.

A correct difference of longitude, it would appear, is not to be expected, by comparing the time of observation with that in the tables; it therefore becomes necessary to have a correspondent observation to compare with, or some satisfactory observations taken under a known meridian, about the time; from which the errours of the tables may be found. Correspondent observations should, however, be obtained if possible: but it must not be supposed, that even these will give a correct difference of longitude, unless observed at both places, under the like favourable circumstances, and with telescopes of the same powers.

Most favourable circumstances.

The air being clear; the planet so high as to be out of the thick atmosphere, and to make the position easy; the telescope sheltered from the wind, and steady; neither moonlight nor twilight, and the satellite not near the body of the planet: An eclipse observed under such circumstances will, I apprehend, be as perfect as it well can be; and a correspondent observation, taken under the like circumstances, will give a correct difference of longitude of the two places, provided the eclipse be observed with telescopes of the same powers.

Much affected by unfavourable circumstances.

Taking the eclipses in the following tables observed under these favourable circumstances as the standard, and comparing their results, as given in the last column, with those of the others, it will be found, how much the latter are affected, by the eclipses having been observed when the atmosphere was hazy, or the planet very low, during twilight, or when the moon was near the planet, or the satellite not far from the body of Jupiter; and that even if correspondent observations had been taken, under favourable circumstances, at a known meridian, the difference of longitude given by the comparison would have been far from correct: the same eclipse observed at two places, under similar unfavourable circumstances, would possibly give a result near the truth; as the observations at both places would be affected in the same way, but probably not in an equal degree, as it is not likely there would be exactly the same degree of haze, the same strength of twilight, &c. at both places; and therefore those observations taken under the same *favourable* circumstances can only be relied upon with certainty.

Difficult to get telescopes alike.

It may not be an easy matter to have telescopes at both places of precisely the same powers for these observations: at Madras we had two telescopes in use, constructed at the same time, in appearance precisely alike, and intended by Dollond to have been so in all respects; yet on repeated trials, one was found to have a decided advantage of several seconds over the other, showing the emersions sooner, and the immersions later by that quantity. In order to do away the errour arising from a difference in the powers of telescopes, *immersions* and *emersions* should be observed at both places; the difference of longitude will be as much greater than it ought to be by one series as less by the other, but the medium will be the correct difference of longitude of the places: it is possible also there may be some difference in the

Both the immersion and emergion should be observed.

Difference in the eyes.

at the other, a result very different from the u
obtained.

SCIENTIFIC NEWS.

Parkinson's Organic Remains.

THE first volume of Mr. Parkinson's Organi
of a former World, which I have noticed in Vol.
has been some time before the public; and I ha
satisfaction of announcing the second, which is
ferior to its predecessor. I trust it will be a sub
tification rather than disappointment to my read
that its indefatigable author has not been able
labours on the fossil remains of the animal worl
present volume, and that he cannot even pledge
limit them to a third; as he very judiciously me
der it as complete as possible, without admitting
fluous matter. In his arrangement he nearly fo
lerius, who, after treating of vegetables, procee
corals, worms, shells, insects, amphibia, fishes,
quadrupeds up to man. The present volume
to zoophytes, many figures of which are given
plates, executed with much elegance, and the g
of them coloured after nature.

the cavities of which are divided by lamellæ disposed in a stellular form. The animal, which in the recent coral fills these cavities, was first depicted by Donati in the 47th volume of the Philosophical Transactions, p. 105, Pl. IV, and in the Natural History of the Adriatic Sea by the same author. Its feet are in considerable number, and terminate externally in two conical productions, which, being placed on each side of every one of the lamellæ that give the stellular form to the cavity of the coral, serve to affix the animal to the circumference of its cell, and may with propriety be considered as the instruments, by which the little animal forms the lamellæ themselves. The bases of these conical productions unite and form round bodies, which possess somewhat of the figure and of the properties of a muscle: they undoubtedly serving to lengthen or shorten the feet, and also most probably to regulate the force, with which they clasp the lamellæ, on which they exert their plastic powers. The other ends of these round bodies terminate in small cylindric tubes, which are attached to the shell of the animal, in the centre of which is seen its head, capable of moving with great quickness, and ornamented with several rays, which are most probably the arms or claws with which it seizes and secures the animalculæ on which it feeds.

Manner in which the coral is formed.

Attributing the formation of these corals to the operations of the madreporean or medusean polype, let us endeavour to trace the little architect through its wonderful labours. Agreeable to the observations of Donati, each of the legs, as he terms them, of the polype are provided with two processes, which are applied to each side of one of the perpendicular laminæ, whilst a muscular pyriform body, attached to the other end of the leg, gives to it the power of employing that motion which is necessary for the accomplishment of its task. The young polype, disposed on an appropriate spot, may be considered as completing its operations by two distinct processes: the secretion and separation of carbonate of lime from the sea water, conveyed through the pyriform body; and its deposition, at its moment of secretion, by the two small processes, where the economy of the animal directs. Proportioned to the number of legs possessed by the infant animal was probably the number of perpendicular

curious fabric have derived its fashion from th
form of this minute and wonderful animal.

That the formation of these turbinated mad
have been thus effected does not appear difficul
Neither is it difficult to understand, that whe
had attained its full extent of growth, the conti
labours would produce, not a body of a conic
cylindrical form. Nor does it appear unlikely
any accidental circumstance change its horizo
a proportional deflection from the straight li
occasioned; and a coralline body of a curved f
duced. Specimens of both these forms, it h
remarked, are frequently found.

Reports of the Preventive Medical Instituation.

THE long expected Reports of the Preven
Institution at Bristol, have been left by the l
does in some degree of forwardness. They
pleted and published as soon as possible, by M
Dr. Hook. The former of these gentlemen h
geon to the Institution from its first commen
the latter has been connected with it since t
March, 1804.

Life of Dr.

Dr. Hook is likewise about to publish a Lif

Magazine. As this monthly publication, being of recent date, may be unknown to many of my readers, I shall just inform them, that its object is, to communicate general and useful information on the different subjects connected with trade, commerce, and manufactures.

St. Thomas's and Guy's Hospitals.

Medical and surgical lectures.

The spring courses of Lectures at these adjoining Hospitals will commence as usual the 1st of February, viz.

At St. Thomas's.

Anatomy and Operations of Surgery, by Mr. CLINE and Mr. COOPER.

Principles and Practice of Surgery, by Mr. COOPER.

At Guy's.

Practice of Medicine, by Dr. BABINGTON and Dr. CURRY.

Chemistry, by Dr. BABINGTON, Dr. MARCET, and Mr. ALLEN.

Experimental Philosophy, by Mr. ALLEN.

Theory of Medicine and Materia Medica, by Dr. CURRY and Dr. CHOLMELEY.

Midwifery, and Diseases of Women and Children, by Dr. HAIGHTON.

Physiology, or Laws of the Animal Economy, by Dr. HAIGHTON.

Occasional Clinical Lectures on Select Medical Cases, by Dr. BABINGTON, Dr. CURRY, and Dr. MARCET.

Structure and Diseases of the Teeth, by Mr. FOX.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a *Complete Course of Medical and Chirurgical Instructions.*

Errata in our last Number.

Page	line	
2	13	*For* E and B, *read* E A B.
	12 from bot.	*For* and $\frac{1.}{\text{rad.}}$, *read* varies inversely as radius.

METEOROLOGICAL JOURNAL

For JANUARY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Ma
in the STRAND, LONDON.

DEC. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Night.	Day.
27	32	33	34	30	29·44	Cloudy	Rain
28	34	37	39	35	29·58	Rain	Ditto
29	36	39	40	37	29·50	Ditto	Ditto
30	38	39	43	33	29·49	Ditto	Cloudy
31	34	38	40	35	29·59	Ditto	Rain
JAN.							
1	36	37	40	35	29·60	Ditto	Ditto
2	37	36	39	30	29·47	Ditto[*]	Ditto
3	31	30	32	28	29·33	Ditto	Snow
4	30	32	33	32	29·63	Ditto	Rain
5	37	43	44	42	29 67	Fair†	Ditto
6	42	42	44	38	29·50	Ditto	Cloudy
7	40	39	44	36	29·32	Ditto	Rain
8	40	40	42	36	28·82	Rain	Ditto
9	40	40	44	34	29·37	Fair	Ditto
10	38	39	43	36	29·20	Cloudy	Ditto
11	38	33	40	33	29·38	Ditto	Fair
12	35	32	36	31	29·49	Ditto	Ditto
13	33	34	38	28	29·69	Ditto	Rain
14	30	32	36	28	29·78	Ditto	Cloudy
15	30	28	30	26	29·75	Ditto	Snow
16	28	27	30	27	30·03	Ditto	Cloudy
17	28	27	28	21	30·06	Fair	Ditto
18	23	23	27	20	29·88	Ditto	Fair
19	24	29	30	29	29·72	Snow	Snow‡
20	28	30	32	25	29·48	Ditto	Foggy
21	32	32	33	29	29·46	Ditto	Cloudy
22	32	32	33	21	29·12	Ditto	Snow
23	22	31	33	30	29·67	Sleet §	Fair
24	32	33	40	33	29·40	Rain	Rain
25	36	33	39	32	29·43	Ditto	Cloudy
26	44	43	46	40	29·22	Cloudy	Fair

[*] Snow at 10 P. M. heavy snow all night.

† The constellation Orion brilliant. Very black in the north.

‡ At 1 P. M. appearance of change, rain freezing as it fell. The thermometers in a few minutes covered with ice, succeeded by rain and snow all the night.

§ At 11 heavy snow.

THE N
PUBLIC
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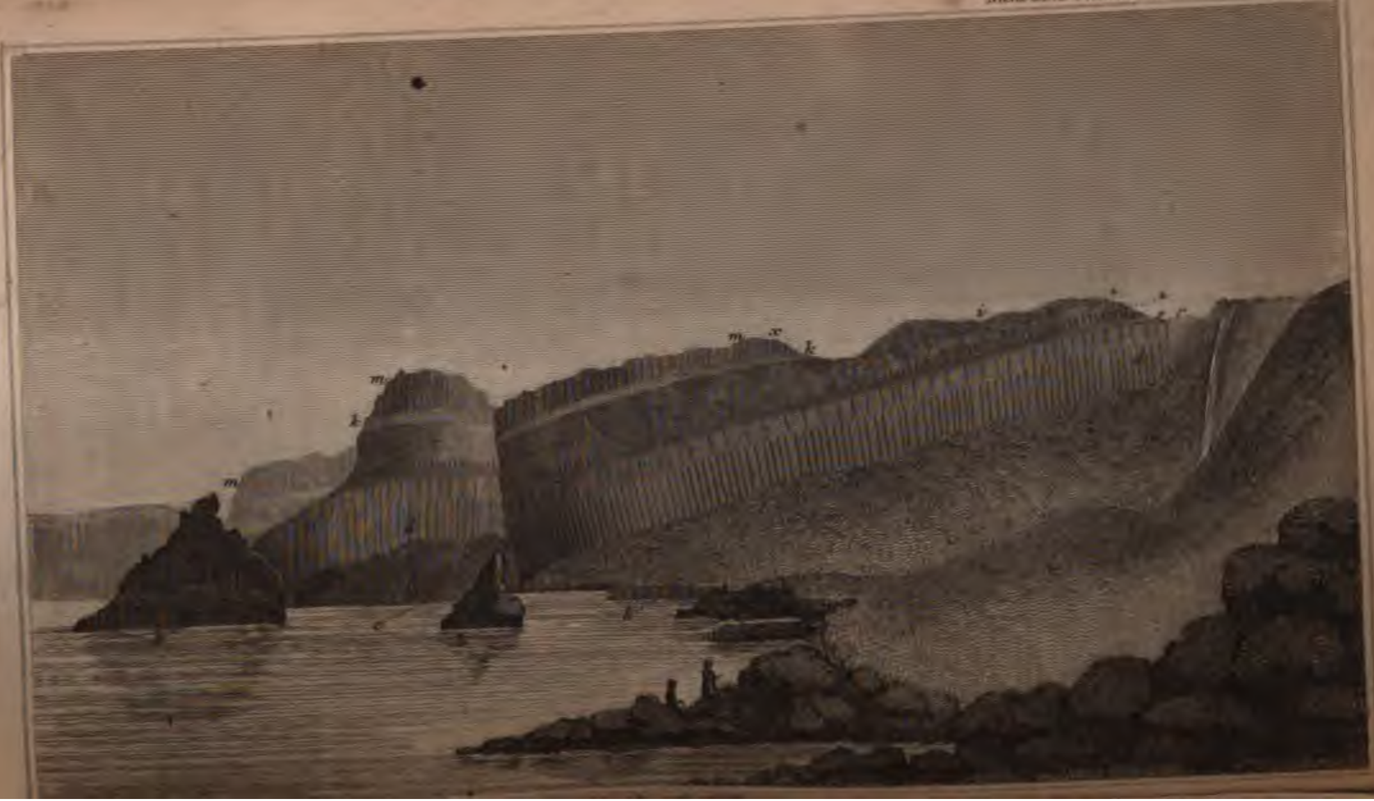
Nicholson's Philos. Journal. Vol. XIII. Pl. 3. p. 16.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1809.

ARTICLE I.

A Letter on the Alterations, that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry and Antrim. Addressed to HUMPHRY DAVY, *Esq. Sec. R.S. By* WILLIAM RICHARDSON, *D. D.*[*]

SIR,

The basaltic country near the Giant's Causeway important to geology,

I Request you will be so good as to lay before the Royal Society the following observations on the Natural History of that part of Antrim, (contiguous to the Giant's Causeway,) which you and I examined so carefully together. I know not any country, that deserves so well to have its facts faithfully recorded; from the important conclusions to which they lead.

The basaltic area (taken in its whole extent) comprehends the greater part of Antrim, and the east side of Derry to a considerable depth.

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[*] Philos. Trans. for 1808, p. 187.

and affords much facility to its study.

In a geological point of view, Nature* has been very kind to this district, for not content with assembling together in a small space so many of her curious productions, and arranging them with more regularity and steadiness than in any other country described, she has condescended occasionally to withdraw the veil, and lay herself open to view, often exhibiting a spectacle equally gratifying to the admirer of magnificence, and to the curious naturalist, who can here, by simple inspection, trace the arrangements, which are to be discovered elsewhere only by penetrating beneath the surface of the earth.

Particularly northward and on the coast.

As soon as we enter the basaltic area, we begin to perceive traces of these arrangements; as we advance farther north, they increase; and in the tract near the shore, and especially at the island of *Rathlin*, which seems to have come fresher from the hand of nature than the rest of our area, the stratification of the whole is perfectly visible, and the nature of the several strata laid open to us at their abrupt and precipitous terminations.

To the southward less distinct.

To the southward we perceive the distinctive features abate, and wear away; the basaltic stratification indeed remains, but is no longer displayed to us in the same manner; the neat, prismatic, internal construction of the strata, which occurs so frequently on and near the coast, is scarcely to be met with at a distance from it; a rude columnar appearance is all we find, and that but rarely.

O the north strata completely open.

It is at the periphery of our area, and especially at its northern side, that every thing is displayed to the greatest advantage; here we have perpendicular façades often continuous for miles, and every separate stratum completely open to examination.

Four most striking parts.

Of these façades, four are more distinguished by their grandeur and beauty than the rest, *Magilligan Rock*, *Cave Hill*, *Bengore*, and *Fairhead*.

The two former are at the extreme points of the northwest diagonal of our area, and nearly forty miles asunder;

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* By the word Nature, which frequently occurs in the course of this Memoir, I always mean, according to Ray's definition, the wisdom of God in the creation of the world.

they are at the summits of mountains, and accessible by land.

The precipitous faces of *Fairhead* and *Bengore*, to which I had the pleasure of attending you, and which are visible only from the sea, are the most beautiful, and the most curious; for the strata, which at *Magilligan* and *Cave Hill* are all nearly similar, at *Fairhead* and *Bengore* are much diversified. Of *Fairhead* I have already published an account in Nicholson's Journal, for December, 1801, quarto series, and I now propose to execute an intention, which I have had for some years, of giving a minute account of *Bengore*.

Fairhead.

Bengore.

I am aware, that it will be extremely difficult to convey a clear and adequate idea of an assemblage of 16 strata, (for such is the number of which our promontory is composed), appearing and disappearing at various altitudes, yet retaining each its own proper place, and forming together a most beautiful and regular whole, though never considered as such before.

But as I have the aid of very correct views of the most important parts of the façade, to the accuracy and fidelity of which I have already obtained your testimony—I shall venture to proceed, for I am anxious to bring into notice the most complete exposure of the internal structure of a district, that I have seen or read of; as there is little likelihood that any other person will enjoy the opportunities, which I have had for so many years, of exploring this interesting part of our coast, through a turbulent sea, and rapid tides.

Description of the Promontory of Bengore, and its Stratification.

This promontory commences at the termination of *Bush-oot Strand*, where the coast, the general direction of which for several miles had been due east and west, turns to the north-east, and, after being cut into several semicircular bays, deflects to the S. S. E., and near the old castle of *Dunseverick* resumes its former rectilineal and nearly eastern direction.

Bengore described.

The

covered with grass.

perpendicular, the several strata are easily
from each other, but where the slightest obliq
a grassy covering is formed, that effectually co
neath it; hence the face of the precipice seems
sified; the columnar strata in some places onl
detached groups of pillars, while in others they
sive colonnades.

General circumstances.

I shall now state the appearances as we a
coast the promontory from the westward, not
first view of the precipice every thing that may b
as general, and reserving (as I did with you) f
in the contrary direction, a detailed account
taken separately.

The strata more inclined than the surface.

The first circumstance, that occurs to the
server on his approach, is, that although both
tory itself, and the strata composing it, ascend
ward, yet it is not in the same angle, the strat
inclined to the horizon than the line tracing
of the promontory, a fact which I shall accou
ward.

Strata.

From the *Black Rock* to the *Giant's Causew*
mile) the materials, and their arrangement, ar
those of the coast to the westward, viz. strata
salt, generally separated by thinner strata of a

between the *Giant's Causeway* and *Dunseverick* I now limit myself, as all the strata composing it emerge between these two points.

Rise and fall of the strata.

As we proceed along the coast from the *Giant's Causeway* eastward, we perceive the whole mass of strata ascend gradually, culminate at the northern point of the promontory, and then descend more rapidly, as the land falls away to the south-east, until having traced them across the face of the precipice we see them immerge separately at and beyond *Portmoon whyn dikes.*

Whyn dikes.

The western side of the promontory is cut down perpendicularly, by eleven *whyn dikes*; the intervals between them are unequal, but they all reach from the top of the precipice to the water, out of which some of them again emerge in considerable fragments; they are all constructed of horizontal prisms, which are strongly contrasted with the vertical pillars of the strata through which they pass.

One of the dikes at *Port Cooan*, on *Bengore*, half a mile from the *Giant's Causeway*, is very beautiful; an insulated rock about 160 feet high, and 20 in diameter, stands perpendicular in the middle of a small bay; the main body of the rock is similar to the contiguous consolidated masses; but on the east side a singular whyn dike is joined to it, composed (as they often are) of several walls agglutinated together, with wall-like fragments of other parts of the dike emerging at their base; the solid mass of dike is seen cutting down the precipice to the southward at 150 yards distance.

Depressions of the Strata.

Depressions of the strata.

Soon after we have passed the last of our whyn dikes at *Port Spagna*, (a name derived from a vessel belonging to the Spanish Armada having been driven ashore in that creek), we discover a new and curious circumstance, viz. that the western half of the promontory has sunk or subsided between thirty and forty feet, without the slightest concussion or derangement of the parallelism of the strata.

Two other depressions appear as we proceed onwards, one at *Portmoon*, and the other at the angle where the promontory begins to project from the rectilineal coast; these however are far less considerable in thickness than the preceding, neither of them exceeding five feet.

Such depressions occur at the collieries near *Ballycastle*, and generally on one side of a whyn dike. We have also at *Seaport*, two miles west from the *Giant's Causeway*, a dike, oblique and undulating, with a depression of the strata of about four feet on one side; but on *Bengore* promontory our dikes are unaccompanied by depressions of the strata, and where we have depressions, we do not find a trace of a dyke.

The portions of this extensive façade, which I have selected for explanatory views, are *Portmoon*, in or near which most of the strata emerge, and *Pleskin*, where the strata culminate, each of these views too exhibits one of our depressions, but in that of *Pleskin*, the first apparent depression is purely an optical effect arising from the position of my friend Major O'Neal, of the 56th, who took his view from the water.

Enumeration of the sixteen Strata that compose the Promontory of Bengore, taken in their regular Order, and counting from above.

Enumeration of the strata.

The country immediately to the southward of *Bengore* is like the Promontory itself a stratified mass, accumulated to the summits of *Craigh Park* and *Croaghmore*, the first five hundred and the second seven hundred feet high; but with these strata I have nothing to do, limiting myself to those alone of which the promontory is formed, and which are exhibited in its façades.

The uppermost of these commences near half a mile to the eastward of the angle, where the coast, deflecting from its due east and west course, turns to the north-west, and begins to form the promontory.

So far the course of this stratum is to appearance perfectly horizontal, for the strata all ascending to the north, the intersection of their planes with the plane of the sea must run east and west, that is, in the present case it coincides with the direction of the coast.

But when the coast changes its direction, this coincidence ceases, and the façade (that is the vertical section of the coast)

coast) losing its east and west course, its strata must appear to ascend towards the point it turns to; therefore the strata at *Portmoon*, and along the north-east side of the promontory, should ascend obliquely along the façades, as they actually do.

First Stratum, (*m*). See Pl. V.

1st stratum. The stratum I commence with forms the whole façade, from its first appearance until it reaches the promontory; it consists of massive pillars rather rude, and about sixty feet long, its course for half a mile (as I have stated) seems horizontal, but on the face of the promontory it ascends, and continues to rise uniformly until it reaches the summit, which it lines as far as *Portmoon*, on the south side of which it loses some of its thickness, then suddenly disappears and vanishes from that façade, receding westward in the form of a stony ridge, and is seen no more.

Second Stratum, (*k*).

2d stratum. The stratum upon which the preceding rests is red as brick, and about nine feet thick; it appears in spots and patches just above high water mark, so long as the incumbent stratum continues horizontal, but when that rises obliquely, the second ascends with it; it is now completely displayed, and having supported the preceding in its course to the summit, vanishes with it (at *x* in the view of *Portmoon*,) and is seen no more.

These ochreous matters, so common in all basaltic countries, according to Mr. F. St. Fond's opinion, were once pure basalt, but have undergone some chemical process of nature we are unacquainted with, by which their colour has been changed.

Third Stratum, (*i*).

3d stratum. The next stratum is the last of those composing the promontory which appears beyond it; for so long as the first and second continue their horizontal course toward *Bengore*, this third accompanies them, showing its upper surface between high and low water mark; but when it ascends along with the others across the façades, it displays its whole thickness, above fifty feet.

Irregular prismatic basalt.

This stratum is of that variety of basalt I have on different occasions distinguished by the name irregular prismatic; it resembles the columnar basalt in grain, but differs from it totally in principle of internal construction, for its prisms are small, not articulated, and indifferent as to the position of their axes, which is perpetually changing.

The irregular prismatic basalt accompanies the columnar in most countries, as at *Pont du Baume*, at *Trezza*, at *Bolsena* in the *Sound of Mull*, and at *Staffa*. In *Antrim* it is very common; and here is a striking resemblance between the rock crowning the celebrated columns at *Staffa*, and a stratum covering a very neat colonnade at *Craigahullar*, near *Portrush*.

This stratum (as is well exhibited in the view of *Portmoon)* is scolloped off irregularly from the point where it becomes superficial (*x*), until it completely disappears at (*z*); a thin stripe of its lower edge alone is ever resumed again.

Fourth Stratum, (*h*).

4th stratum.

The next three strata will require only very short descriptions; the fourth is about seven feet thick, entirely columnar, the pillars small, but not neat; they appear very white from a thick covering of *byssus saxatilis*, which shows a great predilection for this stratum.

Fifth Stratum, (*g*).

5th stratum.

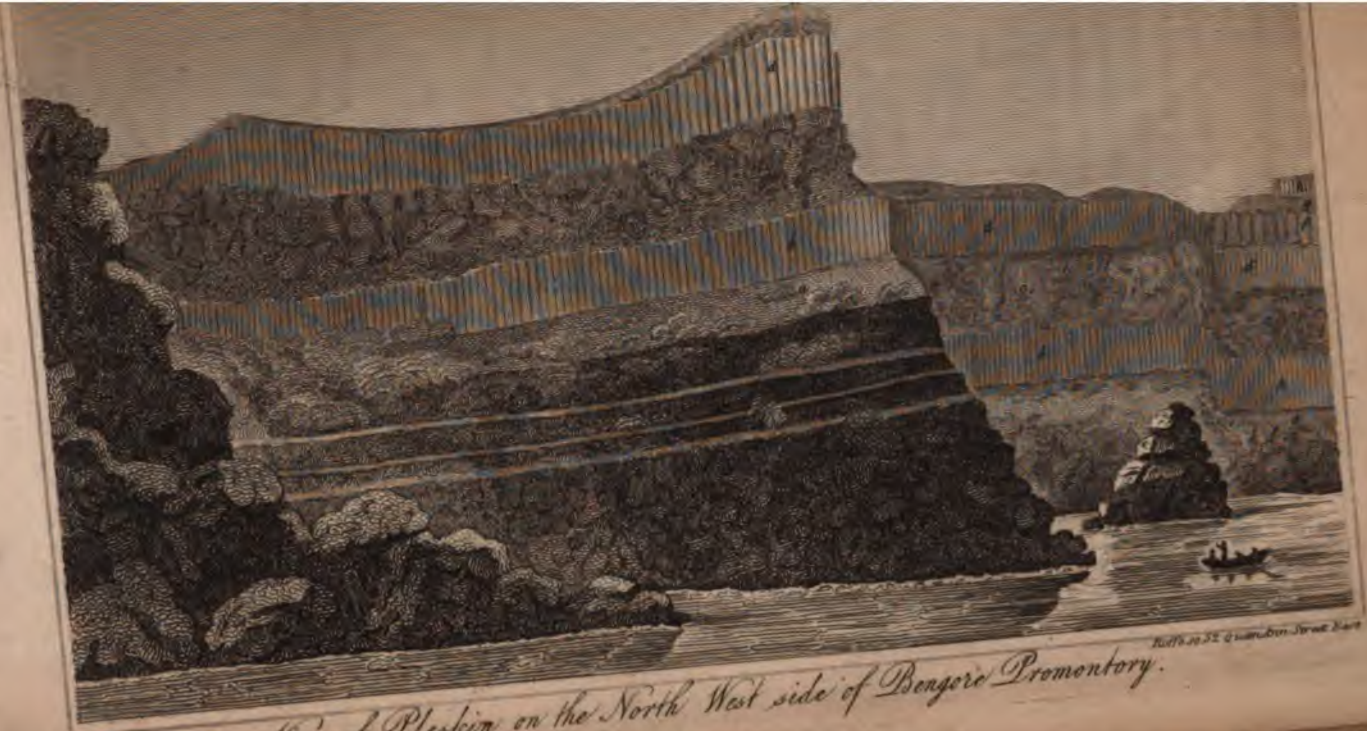
This stratum is ochreous, and more of a slate colour than any of the other red strata; as it is friable, it soon acquires a grassy coat, through which it is only in spots that it shows its proper colour; it is about eight feet thick.

Sixth Stratum, (*f*).

6th stratum.

This stratum is composed of rude massive pillars so coarsely formed, that on the least abatement of perpendicularity the columnar form can scarcely be traced. This stratum is about ten feet thick, it forms the vertex of the

beautiful



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View of Pleskin on the North West side of Bengore Promontory.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS
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beautiful conical island *Beanyn Daana*, and is marked in the views (*f*)*.

These lead to important facts.

These last strata, though they have nothing very remarkable in themselves, nor contribute much to the beauty of the façade; yet they exhibit one of the most important facts I am acquainted with in natural history, and which, when attentively considered, throws much light on the nature of the operations performed upon our globe since its consolidation, and leads us irresistibly to conclusions extraordinary and unexpected.

The fourth, fifth, and sixth strata reach the top of the precipice, and vanish together at the waterfall in the north-west corner of *Portmoon*. When they come to the surface, they turn inland to the westward in long stony ridges; these obstruct the course of the waters in their descent along the inclined plane, formed by the surface of the promontory, and throw them over the precipice in a cascade highly beautiful after rain.

On the façades to the north-west not a trace of them appears, these being entirely formed by the lower strata, which I have not yet noticed; but at the distance of a mile, at the great depression (already mentioned), the fourth, fifth, and sixth strata, with a narrow stripe of the third, suddenly appear, in their regular posts, their proper order, and with all the characteristic marks peculiar to each separate stratum.

In the interval between the depression at *Pleskin*, and the *Giant's Causeway* (about a mile), these three strata often appear in a desultory way on the summit of the precipice, wherever it is of sufficient height to receive them, always preserving their usual thickness, their characters, and their order: so that a person master of the order I am detailing, as he approaches a rising point of the precipice, can tell its strata, and their order, before he is near enough to distinguish them.

Seventh Stratum, (*d*).

7th stratum.

The rude and massive pillars of the sixth stratum pass into the neater and much longer columns of the seventh, without interrupting the solidity or continuity of the material; exactly as a down held hand appears to separate into fingers. The thickness of this stratum, that is the length of the pillars of which it is formed, is fifty-four feet; it is marked (*d*) in the two views, and in its passage across the face of the precipice displays more beautiful colonnades than any of the others.

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* The view of Pleskin will be given in our next.

This seventh stratum emerges from the beach immediately behind the south-east point of *Portmoon*, and where it first shows itself in that bay has its lower edge raised only a few feet above the water; it forms the upper frustum of the larger of the two conical islands, ascends obliquely along the face of *Portmoon*, and continues to rise until it composes the upper range in the beautiful façade, properly called *Bengore Head.* This is properly the most magnificent of all, its convexity towards the sea producing a fine effect. The lower edge of this stratum, that is the line forming the base of its pillars, has here, as at *Pleskin*, attained the height of three hundred feet above the water.

This seventh stratum, like those above it, also suffers an interruption; for after having exhibited itself to such great advantage at *Bengore*, the extreme northern point of the promontory lowers, and this stratum disappears for about one third of a mile; as the promontory rises, it is resumed again in great beauty at *Pleskin*, and is interrupted no more; we scarcely ever lose sight of it until we reach *Port Noffer* (the next bay to the Causeway); here, for want of perpendicularity it is little seen, and is finally lost over the Causeway, we know not well how.

Disappears: is resumed again; and at last vanishes.

Eighth Stratum, (*c*).

8th stratum.

The next stratum is of the same variety of basalt with the third, that is, irregular prismatic; it is fifty-four feet thick, and in the views distinguished by the letter (*c*): where it emerges at the south-east corner of *Portmoon*, it is quite accessible by land, and affords the best opportunity I know for examining this species of basalt, as it is there very neat.

There is little more of this stratum seen in the façade of *Portmoon* for want of perpendicularity, but it forms the lower frustum of the great conical island *Beanyn Daana*, and the whole of the smaller, except the base; it is well displayed over the remainder of the precipice, it forms the intermediate stratum between the magnificent colonnades at both *Bengore* and *Pleskin*, and finally is lost just over the *Giant's Causeway*. Large globular fragments have fallen from it, and are scattered about the causeway. Beanyn Daana.

Ninth Stratum, (*b*).

This stratum is forty-four feet thick, that being the exact length of the neat pillars composing it; at its emersion it forms the bases of the two conical islands in *Portmoon*, and is no more seen in that bay, but immediately to the northward it begins to show itself in colonnades and groups, some of them resembling castles and towers. 9th stratum.

It ascends along the precipice obliquely, like those above it, forms the lower range at *Bengore* and *Pleskin*, from which last it dips to the westward regularly, composes the group at *Port Noffer*, called the *Organs*, seen from the causeway, and finally at its immersion, or intersection with the plane of the sea, it forms the beautiful assemblage of neat pillars, so long distinguished by the name of the *Giant's Causeway*. The Organs.

At these two intersections, each of them accessible by land and water, the prisms exactly resemble each other in grain, size, and neatness; the interval between them is full two miles, through great part of which this stratum is displayed at different heights; it culminates between *Pleskin* and *Bengore*, with its lower edge more than two hundred feet above the water.

We see now what a diminutive portion of our vast basaltic mass has, until lately, monopolized the attention of the curious; and even after it was discovered, that we had many other, and much finer collections of pillars on the same promontory, it never occurred to those who were preparing to give accounts of them to the public, to examine whether these were mere desultory groups, or detached parts of a grand and regular whole, which a more comprehensive view of the subject would soon have laid open to them.

Tenth Stratum, (e).

10th stratum.

The stratum upon which the pillars of the preceding rest is ochreous, red as minium, and about twenty feet thick; it is scarcely seen at *Portmoon*, a patch alone of its surface being distinguishable under water at low tide; but immediately to the northward it shows itself, and from its bright colour makes a conspicuous figure across the face of the precipice in a course of more than a mile and a half; its last appearance to the westward is at *Rovinvalley*, the opposite point of the bay from the *Giant's Causeway*, from which we have a good view of it. The final dip and immersion of this tenth stratum, as well as its emersion, are lost for want of perpendicularity.

Rovinvalley.

11th to 16th strata.

The six remaining strata are all similar in material, but differing much from each other in thickness; they are all of that description called tabular basalt, sometimes showing a faint disposition to assume a columnar form at their edges, and always separated from each other by ochreous layers.

Not so distinct as the others.

These six strata are not so perfectly distinct as those above them, for sometimes we think we can count seven, and again not more than five; nor does each of these preserve the same thickness through their whole extent, for they are deeper towards the northern point, where they culminate, forming by themselves a perpendicular façade near two hundred feet high, but they grow thinner as they recede from this centre.

The jets of black rock in the view of *Portmoon* are the emersions of these strata; their last appearance on the west side is at *Rovinvalley*, where they strongly display the inclination of their strata (the same with all the rest) to those approaching from the westward; their final immersion is lost for want of perpendicularity.

I shall now proceed to select from the great mass of facts, that are exhibited on the face of *Bengore* promontory, and occur in the contiguous basaltic country, such as seem applicable to geological questions, and likely to throw light on such subjects.

Facts applicable to geological Questions.

Geological facts.

1. Every stratum preserves accurately, or very nearly, the same thickness through its whole extent, with very few exceptions.

The strata uniform in thickness.

2. The upper and lower surface of each stratum preserve an exact parallelism, so long as they are covered by another stratum; but when any stratum becomes the superficial one, its upper surface is scolloped, or sloped away irregularly, while the plane forming its base continues steady, and rectilineal; but the parallelism of its planes is resumed as soon as another stratum is placed over it.

Both surfaces parallel, unless one has been exposed to the air.

3. The superficial lines bounding the summit of our façades, and our surface itself, are unconnected with, and unaffected by, the arrangement of the strata below them.

The line of the surface not governed by the strata below.

4. Nature, in the formation of her arrangements, has never acted upon an extensive scale in our basaltic area, (at least on its northern side, where our continuous precipices enable us to determine the point with precision,) but changes her materials, or her arrangement, or both, every two or three miles, and often at much smaller intervals.

Materials and arrangement frequently changed.

5. Wherever there is a change of material, as from one stratum to another in a vertical line; or where the change is in a horizontal direction by the introduction of a new system; or where a whyn dike cuts through an accumulation of strata; in all these cases the change is always *per saltum* and never *per gradus*, the lines of demarcation always distinct, and well defined; yet the different materials pass into each other without interrupting the solidity and continuity of the whole mass.

The changes always sudden, without interruption of continuity.

6. The façades on our coast are formed as it were by vertical planes, cutting down, occasionally, the accumulations of our strata; the upper part of these façades is generally perpendicular, the lower steep and precipitous.

Precipices.

7. The bases of our precipices commonly extend a considerable way into the sea; between the water and the foot of the precipice, (and especially near the latter) there is frequently exhibited the wildest and most irregular scene of confusion, by careless observers supposed to be formed by the ruins of the precipice above, which have fallen down; such,

Their bases extend into the sea.

Apparent fragments on them

not fallen down from above.

such, no doubt, was Mr. Whitehurst's idea, when he describes one of these scenes as "an awful wreck of the terraqueous globe."

But a more attentive observer will soon discover, that these capricious irregularities, whether in the form of rude cones, as at *Beanyn Daana*, and the west side of *Pleskin;* or towers, as at the dike of *Port Coom*, and *Castro Levit*, at the foot of *Magilligan* façade; or even spires and obelisks, as to the westward of *Kenbaan*, and at the *Bull of Rathlin;* yet all of these once formed part of the original mass of coast, stratified like it, and their strata still correspond in material and inclination with those in the contiguous precipice.

Perpendicular cliffs not confined to the coast.

8. These vertical sections or abruptions of our strata are by no means confined to the steeps that line our coast; the remaining boundary of our basaltic area has several of them equally grand; and similar abruptions, or sections (though not so deep) are scattered over a great part of our area, and especially on the ridges of our hills and mountains, which are cut down in many places like a stair by the sudden abruption of the basaltic stratum.

The materials on one side of these carried away,

9. Wherever the strata are thus suddenly cut off, whether it be a mass of accumulated strata, as in the façades on our coast, or solitary strata, as in the interior; the materials on one side of the abruption are completely carried away, without a fragment being left behind, while on its other side the untouched stratum remains intire and undisturbed.

I shall not proceed to apply these facts to support, or invalidate, any of the numerous theories, which have given rise to so much controversy, in which I myself (as you know) have borne some part; I shall look to nature alone, without much reference to opinions, and shall endeavour to trace, by the marks she has left behind her, some of the grand operations she once executed on the surface of our globe.

The 3 divisions of history applicable to natural history.

Varro divided the time elapsed since the beginning of the world into three portions, which he distinguished by the names, *prolepticum*, *fabulosum*, and *historicum*.

The first comprehended the period of absolute darkness;

in the second some faint lights were thrown upon the history of its events by fable and tradition; in the third, the historian had the common aids, from which history is usually compiled.

Period of absolute darkness.

The natural history of the world seems to admit of a corresponding division. In the first I include the formation of our strata, their induration, their derangement from the horizontal position in which they seem originally to have been placed, and the operation of cutting them down by so many whyn dikes.

Fabulous period.

In the second division, corresponding to Varro's *fabulosum*, I comprehend the operations performed upon our globe posterior to its final consolidation, and antecedent to all history or tradition; operations therefore that can be established by the visible effects alone which still exist, written in strong characters.

Historical period.

The third division contains the period since we acquired some knowledge of natural history, became acquainted with causes and effects, and able to trace the connection between them.

Modern theorists.

With the operations performed in the first division (corresponding with Varro's *prolepticum)* modern theorists assume that they are well acquainted, able to account for every appearance, and to detail the whole process of original formation. I however shall decline noticing these early processes of nature, and limit myself to the second division of natural history, hoping from the prominent features of my country that remain still undefaced, and from its curious facts, to trace and demonstrate the great effects, that have been produced upon our surface; and though I do not presume to advance farther, I perhaps may assist in clearing the way for future naturalists, and by establishing effects, encourage them to proceed to causes, and help them to discover the powers and agents, by which these grand operations have been executed.

(To be concluded in our next.)

II.

SIR,

Camphor purified by pure potash.

FROM an idea of pure pota
for fixed oils, than the essential
phor as one of the latter in a co
to try its effects on some, whicl
camphor of commerce, was very
siderable empyreumatic smell.
with mixtures of it, different fi
to divide the particles), I found,
small admixture of pure potash

Subcarbonate ineffectual.

pyreumatic smell were detained
bonate does not answer the purp
affinity of potash for oils is less
of carbonic acid.

6 p. camphor, 8 olive oil, 24 sand, and 1 potash, mixed, and the camphor sublimed.

Two drachms of camphor wi
tic smell, and dirty, were mix
eight of sand; after which tw
were added, and heat applied;
than is necessary for its sublin
fectly free from empyreumatic
it generally is.

Substituting linseed oil prod
duct; and supposing, that th
phor are not more liable to r
those employed, which did wi
kali. I take the liberty of sub

III.

An Account of some Peculiarities in the anatomical Structure of the Wombat. By EVARARD HOME, *Esq. F. R. S*[*].

Wombat from Basse's Straits.

A Male wombat was brought from the islands of Basse's Straits, by Mr. Brown, the naturalist attached to Captain Flinders's voyage of discovery. It was entrusted to my care, and lived in a domesticated state for two years, which gave me opportunities of attending to its habits.

Its habits.

It burrowed in the ground whenever it had an opportunity, and covered itself in the earth with surprising quickness. It was quiet during the day, but constantly in motion in the night: was very sensible to cold; ate all kinds of vegetables; but was particularly fond of new hay, which it ate stalk by stalk, taking it into its mouth like a beaver, by small bits at a time. It was not wanting in intelligence, and appeared attached to those to whom it was accustomed, and who were kind to it. When it saw them, it would put up its fore paws on the knee, and when taken up would sleep in the lap. It allowed children to pull and carry it about, and when it bit them did not appear to do it in anger or with violence. It appeared to have arrived at its full growth, weighed about twenty pounds, and was about two feet two inches long.

The koala an other species.

The koala is another species of the wombat, which partakes of its peculiarities. The following account of it was sent to me some years ago by Lieut. Colonel Paterson, Lieutenant-Governor of New South Wales. The natives call it the koala wombat; it inhabits the forests of New Holland, about fifty or sixty miles to the south-west of Port Jackson, and was first brought to Port Jackson in August, 1803. It is commonly about two feet long and one high, in the girth about one foot and a half; it is covered with fine soft fur, lead coloured on the back, and white on the belly. The ears are short, erect, and pointed; the eyes generally

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[*] Abridged from the Philos. Trans. for 1808, p. 304.

IV.

On the Changes produced in Atmospheric Air, and Oxigen Gas, by Respiration. By W. Allen, *Esq.*, *F. R. S.*, and W. H. Pepys, *Esq.*, *F. R. S.*[*]

Respiration has been investigated

by many,

but some important points still unsettled.

Residual gas in the lungs often a difficulty.

This obviated.

THE process of respiration, or breathing, is so intimately connected with our existence in life, that from its first moments to the final close, sleeping and waking, this necessary action is constantly maintained; nor can it be suspended even for a few minutes without considerable pain and the utmost danger. This important process has of course excited the curiosity both of ancient and modern philosophers; among the latter we find the distinguished names of Mayow, Priestley, Goodwin, Menzies, Spallanzani, Scheele, Lavoisier, and Davy, whose successive labours have thrown great light upon this difficult subject, and prepared the way for farther investigation; but it is impossible to take a review of what has already been done, without perceiving, that some important points were by no means satisfactorily settled; an accurate method of separating the different gasses, and ascertaining their exact proportion in any given mixture, was still a desideratum when many of the experiments were made; and it is only of late years that eudiometry has attained its present perfection. The quantity of residual gas in the lungs after a forced expiration was a matter in dispute among former experimenters, some making it one hundred and nine cubic inches, and others only forty; and yet it is of the utmost importance in all calculations upon the effects produced, especially upon small portions of gas, that the state of the lungs should be accurately determined; this constitutes the great difficulty in the investigations. We therefore commenced our labours by constructing an apparatus, in which we are able to respire from three to four thousand cubic inches of gas, conceiving, that in this quantity, the errour arising from the residul gas in the lungs must be so much obviated as to permit the most satisfactory results.

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[*] Philos. Trans. for 1808, p. 249.

Apparatus for the purpose.

The apparatus consists of three gasometers, two of which are filled with mercury, and one with distilled water.

The water gasometer which belongs to the Royal Institution is capable of holding four thousand two hundred cubic inches of gas, and each of the mercurial ones three hundred cubic inches; the apparatus was so arranged, that the inspirations were all made from the water gasometer, and the expirations into the mercurial gasometers alternately. Each of the gasometers is furnished with a graduated scale, and they are all made to range with each other, so that the quantity of gas inspired and expired could be immediately and exactly ascertained: to each of the mercurial gasometers a glass tube is fixed, and made to enter a mercurial bath, from which portions of the expired air could at any time be taken for examination.

By the kindness of our friend Silvanus Bevan, we are enabled to give an accurate drawing of the apparatus.

Description.

Reference to the plate.

Pl. VI, fig. 1. The communication with the water gasometer.

2. A cock so constructed that it might be made to communicate with either of the mercurial gasometers, while at the same time all connexion with the other was cut off.

A. The mouth piece.

3 to 10. Brass cocks.

G. 1, and G. 2. Mercurial gasometers.

S. S. Scales graduated to cubic inches.

M. Mercurial bath.

The large reservoir or water gasometer is not shown in this drawing, it having been so frequently described in chemical works.

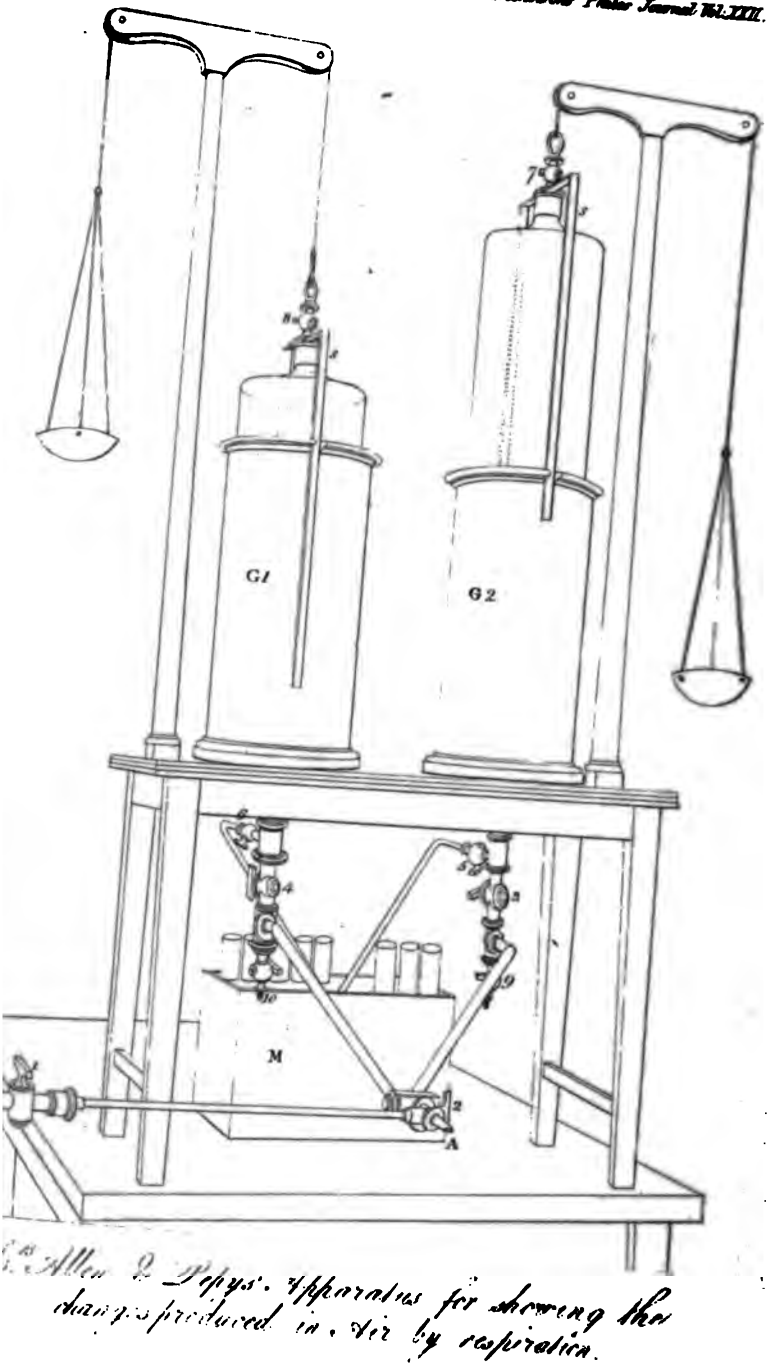
Manner of conducting the Experiment.

Manner of conducting the experiments.

Our first care was, to be certain that all the parts of our apparatus were perfectly air-tight, and this, from the nature of it, was very easily ascertained; we agreed, that the breathing should always be performed by one of us, and the registering &c. by the other, as each would by these means acquire a greater degree of dexterity in performing his part, and the results would be more uniform.

Experiment. The water gasometer being filled with common air to a certain mark upon the scale, and the mercurial ones completely empty; the person to breathe, whom we shall uniformly call the operator, was seated upon a stool, with his mouth even with the tube A, his nose being secured with a steel clip. He made as complete an expiration as possible into the open air, then applying his lips to the tube, and keeping his left hand constantly on the cock 1, and his right hand on the cock 2, he opened the communication with the water gasometer, and made an inspiration; then immediately closing it, he opened with his right hand the cock at 2; and that at 4 being also opened, he expired into the mercurial gasometer G 1; then closing 2, which cut off all communication with the mercurial gasometer, he opened 1, in order to make a fresh inspiration; then closing it, he again opened 2, and expired into the mercurial gasometer; and proceeding in this way, always taking care to shut one cock before the other was opened, the air was made to pass from the water gasometer, through the lungs of the operator into the mercurial gasometer, and this with great ease, as the diameters of the tubes were purposely made large. The scale of the mercurial gasometer was carefully noticed, and when nearly full, the cock 4 underneath was shut off: then, by a signal from the operator, his colleague opened 3, and the expirations were received in G 2. While this was filling, the number of cubic inches in G 1 was registered, a portion saved in the mercurial bath, and the rest quickly expelled. This operation was repeated, until the contents of about twelve or thirteen mercurial gasometers were taken off: the operator always concluding with a strong effort to empty his lungs as completely as possible. The quantity inspired from tha water gasometer was then compared with the quantities expired into the mercurial gasometers, and the difference noted. The following are the results of the first ten experiments.

Nicholson's Philos. Journal Vol. XXII. Pl. 6.



Allen & Pepys'. Apparatus for showing the changes produced in Air by respiration.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

Results of the 1st ten experiments.

No.	Time.	Cubic inches of common air inspired.	Cubic inches of gas expired.	Deficiency.
1.	time not noted	3760	3741	19
2.	11 minutes	3900	3869	31
3.	10½	3624	3620	4
4.	10½	3570	3550	20
5.	11	3685	3653	32
6.	11	3380	3355	25
7.	10½	3180	3141	39
8.	10½	3360	3298	62
9.	10½	3290	3267	23
10.	11	3580	3543	37

·08 of carbonic acid in the last.

In this last experiment we ascertained, that the expired gas contained 8 per cent of carbonic acid.

The breathing nearly natural but rather slower.

The breathing in these cases was as nearly natural as we conceive it possible to be in any apparatus: the operator was scarcely fatigued, and his pulse not raised more than about one beat in a minute; the respirations however were deeper and fewer than natural, amounting only to about 58 in eleven minutes, whereas from repeated observations at different and distant times he makes 19 in a minute. The smallness of the deficiency surprised us very much, as, from the reports of other experimenters, we had been prepared to expect a much greater loss. It might be objected, that the air was rarefied by passing through the lungs; but this was almost immediately counteracted by the mass of quicksilver in the gasometers, which amounted at least to one hundred and fifty pounds; and we have repeatedly noticed, that air under these circumstances has suffered no perceptible diminution by standing for a considerable time; in one case, in which air from the lungs was driven into the mercurial gasometers for twenty-seven minutes, the temperature of the quicksilver at the end of the experiment was not raised half a degree of Farenheit's thermometer. The deficiency, in our opinion, principally arises from the difficulty in bringing the lungs precisely to the same state after, as before the experiment; and it must be recollected, that the operator commenced by a forcible expiration into the open air, but finished by a forcible expiration into the mercurial gasometer.

A greater deficiency expected;

but even this arises principally from air left in the lungs.

Now, although this gasometer was counterpoised by weights in the scale attached to it, yet we can easily conceive, that more resistance might be afforded to the complete evacuation in the latter case than in the former, and consequently the lungs might contain a few inches more after the experiment than before it, which might in some measure account for the deficiency.

11th experiment.

In the eleventh experiment, portions of gas were taken off from each of the mercurial gasometers as they were filled, and these portions being afterward mixed were carefully examined.

Eleventh Experiment.

Barom.	Thermom. Faht.	Time.	Cubic inches of common air inspired.	Cub. in. expired.	Deficiency,
30·4	50°	11 min.	3460	3437	23

All the experiments made with great care,

To prevent repetition, we shall here state, that all the trials were made in the same manner, and with the same apparatus, namely, the eudiometer, described in the Society's Transactions for 1807, in which one cubic inch is divided into one hundred parts*; and that in almost every instance we made two, and sometimes three experiments on the same gas, and derived fresh confidence from the remarkable coincidence and uniformity of the results. No precaution was at any time omitted which appeared to us necessary to insure accuracy.

Component parts of the expired gas,

One hundred parts of the expired gas being agitated with limewater in the eudiometer, the limewater became turbid, and 8·5 parts of the gas were absorbed, which were consequently carbonic acid; the remaining 91·5 parts were treated with the green sulphate of iron, saturated with nitrous gas, as recommended by professor Davy, and afterward with the simple solution of the green sulphate, when 12·5 parts were absorbed, which were consequently oxigen, and the remaining 79 azote.

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* See our Journal, vol. XIX, p. 86.

100 parts of the expired gas therefore consisted of

8·5	carbonic acid.
12·5	oxigen
79·	azote.
100.	

The air contained in the water gasometer, previous to the experiment, being examined by the same tests, consisted in 100 parts of

and of the air previous to respiration.

21	oxigen
79	azote.
100.	

In trying common atmospheric air with limewater, we could never find any quantity of carbonic acid perceptible in the eudiometer of 100 parts.

No carbonic acid perceptible in common air.

Calculation for Carbonic Acid.

$$100 : 8{\cdot}5 :: 3437 : 292{\cdot}145.$$

So that 292·14 cubic inches of carbonic acid gas were given off in eleven minutes, or 26·55 cubic inches per minute, which is almost exactly the estimate of professor Davy.

26·55 cubic inches of carb. acid gas given off every minute.

In this experiment the operator inspired 3460 cubic inches in eleven minutes, and felt himself in a natural state when he left off. Then, as he makes usually under common circumstances nineteen respirations in a minute.

16½ cubic inches taken in at an easy inspiration.

$$11 \times 19 = 209 \quad \frac{3460}{209} = 16{\cdot}5$$

it follows, that he takes in 16½ cubic inches at every easy inspiration.

As all the experiments had been hitherto made upon the lungs of one person, we concluded, that the next should be performed upon our assistant.

Twelfth Experiment.

12th experiment.

Barom.	Therm. Faht.	Time.	Cub. inches of com. air inspired.	Cub. inches expired.	Difference.
30·3	56°	5½ min.	3300	3311	11 increase

Here, as usual, the lungs were exhausted both before, and at the close of the experiment.

An excess of expired air.

The excess of eleven cubic inches, in this case, no doubt, arose from the person not having been in the habit of exhausting his lungs, so that they contained more when he began than when he left off; his lungs appeared to be of greater capacity than those of the usual operator.

Portions of gas were saved from each of the mercurial gasometers as they were filled, which being mixed together, for the average gave the following results:

Component parts of the expired gas.

100 parts of the mixture contained

8·5	carbonic acid
12·5	oxigen
79	azote.
100.	

Calculation for Carbonic Acid.

100 : 8·5 : : 3311 : 281·43.

Consequently 281·43 cubic inches of carbonic acid gas were given off in 5½ minutes.

The carbonic gas is in proportion to the air respired.

In this experiment we meet with a remarkable fact, viz. that as much carbonic acid gas was given off in 5½ minutes, as in the former experiment in eleven minutes; so that it appears, whenever atmospheric air is taken into the lungs, it returns charged with about 8 per cent of carbonic acid. The faster respiration is performed, the more carbonic acid is given off, and consequently the more oxigen consumed: in this instance it was given off at the rate of fifty-one cubic inches per minute.

Thirteenth Experiment.

13th experiment with three times the quantity of air.

We now proceeded to carry on the respiration of common air for a much longer period than usual, and of course on a much larger quantity. The experiment was made by the same operator who had performed all the others, except the 12th. Eleven mercurial gasometers having been filled, taken off, and registered, the operator continued to breathe in the 12th until a mark was made by his colleague upon the scale of the water gasometer, and it was again filled with common air to the usual division on the scale. This occupied but a very short space of time. The operator, without taking his lips from the tube, then filled twelve more of the mercurial gasometers, which were registered as before, and he continued to breathe in the 12th, until the water gasometer was again replenished; eleven more were then filled, and portions saved from each: the experiment was completed by a forcible expiration of 166 cubic inches into the 12th; and this last portion being left for an hour and a half was not perceptibly diminished in volume.

Barom.	Therm. Faht.	Time.	Cub. inches of common air inspired.	Cub. inch. expired.	Defic.
29.85	68°	24′ 37″	9890	9872	18

The breathing was so nearly natural that the operator was scarcely fatigued, and thought that he could have gone on for a much longer time.

Increase or decrease of air merely accidental.

The smallness of the deficiency, notwithstanding the experiment occupied 24½ minutes, is a striking circumstance, and leads us to suspect still more strongly, that the deficiency principally arises from the impossibility of always bringing the lungs to the same state after forcible expiration.

Component parts of the expired gas.

100 parts of the mixture of expired gas gave
8 carbonic acid
13 oxigen,
79 azote,
———
100

Calculation for Carbonic Acid.

100 : 8 : : 9872 : 789·76.

Carbonic acid.

So that 789·76 cubic inches of carbonic acid gas were given off in 24½ minutes, which gives thirty-two cubic inches per minute. But here it must be noticed that the respiration was more rapid than in the 11th experiment, and a larger quantity of carbonic acid given off in the same time. This agrees with the 12th experiment.

We

In ordinary respiration much air returned unaltered.

We are very much inclined to think, that in ordinary respiration, a great part of the air is returned unaltered, viz. that contained in the fauces, in the trachea, and probably a portion of that in the larger branches of the bronchia. If this circumstance be not adverted to in experiments upon small quantities of air, the results can never be correct. There is even a considerable difference in the quality of the first and last portions of a single inspiration. In some experiments made with a view to this subject, a small quantity of the first portions, given off in a common and natural expiration, was received in a vessel over mercury; on examination, it contained only 3·5 per cent carbonic acid; in other experiments the first portions contain from three to five per cent; while the general average appears by the 11th, 12th, and 13th experiments, to be about eight.

204 cubic inches expired contained ·095 of carbonic acid.

The operator, after rather more than a natural inspiration, expired 204 cubic inches into the mercurial gasometer, making his utmost efforts to press as much as possible out of the lungs. These contained 9·5 per cent of carbonic acid. Here we are to recollect, that these 204 cubic inches contained the first, as well as the last portions; the first portions have been proved to contain only from three to five per cent; consequently the last portions must contain more than appears by the average; that is, more than 9·5 per cent.

It now appeared to us of consequence to ascertain exactly what happened to a given volume of atmospheric air, when it was inspired and expired as often as possible.

*Fourteenth Experiment**.

14th experiment. Air repeatedly respired.

Three hundred cubic inches of atmospheric air were admitted into the mercurial gasometer G 1; the other, G 2, was empty. The nose being properly secured, and the mouth applied to the tube A, as usual, air was drawn from G 1, and by half turning the cock 2, was expired into G 2. This was repeated until the contents of G 1 had been made to pass through the lungs, and transmitted to G 2. The air was then inspired from G 2, and expired into G 1, until G 2 was nearly empty. This was repeated about eight or ten times during three minutes, until respiration became extremely laborious, and the operator desisted.

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* In this experiment there was obviously no occasion to make allowance for the air contained in the tubes and sockets. We find its volume to be eighteen cubic inches.

The whole 300 cubic inches must have passed eight or ten times through the lungs; and we confidently expected, that on examining the air we should have found an unusual proportion of carbonic acid.

Component parts of the respired air.

But 100 parts gave only 9·5 carbonic acid,
5·5 oxigen,
85· azote,
———
100

Here was an increase of six parts in 100 of something which the tests for oxigen would not take up, and also a loss of six per cent oxigen. This seemed to convince us, that under certain circumstances, as during some peculiar alteration in the vital functions, gaseous oxide of carbon, carburetted hidrogen, or some other gas not absorbable by lime water or the tests for oxigen, might be given off from the lungs, and we accordingly determined to repeat Cruikshank's experiments with hyperoxigenized muriatic acid gas.

Gaseous oxide of carbon converted into carbonic acid by hyperoxigenized muriatic acid gas.

We procured the gas from hyperoxigenized muriate of potash by means of muriatic acid, and mixing it with a known portion of gaseous oxide of carbon in a flint stopper bottle, the mouth of which was immersed in mercury for twenty-four hours, the gaseous oxide of carbon was converted into carbonic acid gas, as was proved by its effects upon lime water, which, when both the gasses are pure, absorbs them entirely after they have remained together for twenty-four hours; it was plain, therefore, that we had the means of detecting gaseous oxide of carbon, and doubtless carburetted hidrogen, if any should be contained in the expired gas. From a conviction of the importance of these experiments we were determined to take nothing upon trust.

Fifteenth Experiment.

15th experiment.

We repeated the 14th experiment with a little variation. In this case we employed only one of the mercurial gasometers, Air repeatedly respired.

ters, into which exactly 300 cubic inches of atmospheric air were admitted. The operator, having made an easy expiration, applied his mouth to the cock at the top of the bell glass, and the time being noted, began to breathe; in less than a minute he found himself obliged to take deeper and deeper inspirations; and at last the efforts of the lungs to take in air became so strong and sudden, that the glass was in some danger of being broken against the side of the gasometer. A great sense of oppression and suffocation was now felt in the chest, vision became indistinct, and after the second minute his whole attention seemed to be withdrawn from surrounding objects and fixed upon the experiment. He now experienced that buzz in the ears which is noticed in breathing nitrous oxide, and after the third minute had only sufficient recollection to close the cock after an expiration. This secured the result of the experiment; but he became so perfectly insensible, that, on recovering, he was much surprised at finding his friend and the assistant on the table in the act of supporting him. It was noticed that he made thirty-five inspirations during the experiment. We now examined the air which had been so treated.

The air examined.

100 parts contained 10 carbonic acid,
4 oxigen,
86 azote,
———
100

In this experiment it is remarkable, that the air which had been so often through the lungs should only have furnished 10 per cent of carbonic acid, while the air which passes them but once contains from 8 to 8·5.

Here the oxigen had lost 7 from 21, and the azote had gained 7 upon 79.

We knew by previous experiment*, that every cubic inch of carbonic acid gas required exactly a cubic inch of oxigen for its formation; the ten parts of carbonic acid may therefore be reckoned as oxigen, which would make the constitution of the gas after the experiment $\begin{cases}\text{14 oxigen,}\\ \text{86 azote,}\end{cases}$

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* See the experiments on carbonic acid in the Society's Transactions, or our Journal, vol XIX, p. 225.

whereas before the experiment it was $\begin{cases}\text{21 oxigen,}\\ \text{79 azote.}\end{cases}$

Residuum examined.

Now we did not suppose the residuum of 86 to be all azote, though 79 might be; therefore seven parts appeared to have been added by this unnatural mode of respiring, and we conjectured the addition might be gaseous oxide of carbon.

To ascertain this, we put 40 parts into a flint stopper bottle, and nearly filled it with about 100 parts hyperoxigenized muriatic acid, procured as before, and recently prepared; the stopper being put in, over distilled water, we plunged it into quicksilver, and filled a second bottle in the same way, as a comparative experiment.

Hyperoxigenized muriatic acid gas has no action upon azote.

We next procured some pure azote, by absorbing the oxigen from a portion of atmospheric air by the saturated green sulphate and simple green sulphate as usual; 40 parts of this azote were mixed with the same proportion of the acid gas as in the other experiment, and the whole suffered to stand for forty-eight hours; at the end of this time the azote was examined, by washing it first in distilled water, and afterward in the eudiometer with the tests for oxigen; and there were still exactly 40 parts left; proving that the hyperoxigenized muriatic acid gas has no action upon azote.

The residuum azote.

We then examined the bottles containing the residuum from the air that had been so often respired, and found that it had not experienced the slightest change; it was therefore plainly azote; and on reflection, it occurred to us, that if a certain proportion of oxigen had been absorbed or lost in any way, while the azote remained unaltered, there must be an increased proportion of the latter.

Now we knew exactly both the bulk and the constitution of the air before the experiment; but it was impossible to know the bulk or volume after the experiment otherwise than by calculation.

The 200 cubic inches of atmospheric air before the experiment contained 21 oxigen and 79 azote in 100 parts, making the total quantity of oxigen 63 cubic inches,

azote 237

300

The lungs absorb little if any azote.

Now if the lungs be capable of fixing permanently any azote from the atmosphere, it appears by our experiments, that the quantity must be very minute, seeing that in the 11th, 12th, and 13th experiments, it did not disturb the proportion of azote, as shown by the eudiometer; we shall therefore in the present instance assume the volume of azote after the experiment at 237 cubic inches, as before.

But after the experiment, every 100 parts consisted of 86 parts azote, and 14 oxigen, either in the form of carbonic acid, or free.

86 : 14 : : 237 : 38·58.

Therefore the total quantity of oxigen left after the experiment would have been 38·58 cubic inches.

Then 237 azote + 38·58 oxigen = 275·58;

the quantity of gas after respiration would therefore have been 275·58 cubic inches.

300 — 275·58 = the loss of oxigen, or 24·42 cubic inches.

Oxigen absorbed during respiration.

It appears, therefore, that 24·42 cubic inches of oxigen had been absorbed by the system under the circumstances of this experiment.

Reviewing the 14th experiment, it appears, that the gas after respiration contained 85 per cent azote, and 15 per cent oxigen, either in the state of carbonic acid, or free.

State of the air in exp. 14.

State of the Air before the Experiment.

300 = 237 azote + 63 oxigen.

After the Experiment.

85 : 15 : : 237 : 41·82.

The total quantity of oxigen after the experiment appears to be 41·82 cubic inches.

Then 237 azote + 41·82 oxigen = 278·82.

The total volume after the experiment appears to be 278·82 cubic inches.

300 — 278·82 = 21·18.

Oxigen absorbed.

The loss of oxigen in this case was 21·18 cubic inches.

According to experiments, above 11 oz. troy of carbon emitted from the lungs daily, and 39534 cub. inches of oxigen gas consumed

We are disposed to consider the 11th as a standard experiment relative to carbonic acid gas, because the quantity of air respired in a given time is pretty near the average of the first ten experiments; and because it very nearly agrees with the statement of Professor Davy. In this experiment 292 cubic inches of carbonic acid gas were given off in eleven minutes; the barometer was 30·4, the thermometer 50°, the volume being calculated at the mean, viz. barometer 30, thermometer 60°, will be 302 cubic inches given off in eleven minutes, or 39534 cubic inches in twenty-four hours, supposing the production to be uniform during all that period; and as 100 cubic inches of carbonic acid gas weigh 47·26 grains,

100 : 47·26 : : 39534 : 18683·76;

the weight of the carbonic acid gas amounts to 18683.76 grains; and estimating the carbon in it at 28 parts in 100, according to Lavoisier, or 28·60, as calculated in the experiments on diamond, recorded in the Society's Transactions,

100 : 28·60 : : 18683·76 : 5363·55 grains;

it will follow, that 5363·55 grs. or above 11 oz. troy of solid carbon, are emitted by the lungs in the course of twenty-four hours; and that 39534 cubic inches of oxigen gas are consumed in the same time. But when we consider, that in respiration perfectly natural a much smaller quantity of air can come in contact with those parts of the lungs calculated to act upon it, the proportion of carbonic acid gas given off in natural respiration ought probably to stand considerably lower than in the above estimate; but at all events it will be very considerable.

But this more than the common average.

Sixteenth Experiment.

16th experiment.

Having made so many experiments upon atmospheric air, we now proceeded to ascertain the effects produced upon

Oxigen gas respired.

upon oxigen gas, by the process of respiration. The water gasometer was filled with oxigen gas made from the hyperoxigenised muriate of potash by heat, care having been taken to clear all the tubes, &c. as much as possible of common air, by forcing a quantity of oxigen gas through them. One hundred parts from the water gasometer being treated with the usual tests in the eudiometer, a residuum of only 2·5 was left; so that 97·5 per cent were pure oxigen, and the rest azote.

Pulse quickened,

The register of the water apparatus being noticed, and the operator having prepared himself as usual by a forced expiration, began to respire; his pulse was 72; and at the end of 9 minutes and twenty seconds, the experiment was concluded by a forced expiration, when the pulse was raised to 88.

Barom.	Therm. Faht.	Time.	Cubic Inches of oxigen gas inspired.	Cub. Inches expired.	Deficiency.
29·5	53°	9′20″	3960	3193	67

and natural heat increased.

The operator felt a general glow over the body to the very extremities, with a gentle perspiration; this however went off in a few minutes, and no remarkable deviation from the ordinary state was experienced.

Component parts of the expired gas.

A portion having been saved, as usual, from each of the mercurial gasometers, for an average,

100 parts contained

11 carbonic acid,
83 oxigen,
6 azote,

100

The examination repeated gave the same results.

Calculation for Carbonic Acid.

100 : 11 :: 3193 : 351·23,

consequently, 351·23 cubic inches of carbonic acid gas were formed in 9′20″, or 37·64 cubic inches in a minute.

More carbonic acid formed.

Here it is plain, that a greater quantity of carbonic acid was formed from oxigen than from common air, in the same time, and hence we infer, that one use of azote is to regulate the quantity of oxigen, which shall be taken up in the act of respiration.

Use of azote in the air.

The gas inspired was 3260 cubic inches, and of this 2·5 per cent were azote.

100 : 2·5 : : 3260 : 81·50.

The total quantity of azote in the gas inspired was therefore 81·50 cubic inches.

The quantity of gas expired was 3193 cubic inches, and of this every 100 parts contained six of azote.

100 : 6 : : 3193 : 191·58.

The total quantity of azote in the gas expired was therefore 191·58 cubic inches; but the total quantity of azote before respiration was only 81·50.

191·58 — 81·50 = 110·08;

therefore 110·08 cubic inches were added by the process of respiration, beside what little remained in the lungs after the experiment.

The azote apparently increased during respiration.

Calculation for Oxigen.

The 3260 cubic inches of gas inspired contained 81·50 azote.

Calculation of the oxigen.

3260 — 81·50 = 3178·50,

and consequently the pure oxigen was 3178·50 cubic inches. The 3193 cubic inches of gas expired contained 83 per cent of free oxigen, and 11 per cent in carbonic acid gas, making together 94.

100 : 94 : : 3193 : 3001·42.

The oxigen gas, found after the experiment, was therefore 3001·42 cubic inches, and deducting this from the oxigen before the experiment,

3178·50 — 3001·42 = 177·08.

It appears, at first sight, that 177·08 cubic inches of oxigen were missing, but great part of this may be accounted for, by adverting to the state of the lungs after the experiment.

Portion missing.

The addition to the azote from the air in the lungs.

The addition of 110·08 cubic inches of azote we
der as arising from that portion still retained in th
notwithstanding the forced expiration at the begin
the experiment: and considering that in the 14th a
experiment, where the same air was repeatedly br
the proportion of azote was in the one case 85, and
other 86 per cent; it seems fair to presume, that t
dual air contained in the lungs after a forced expirati
amount in 100 parts to not more than 16 oxigen
azote. Any one who reflects upon the structure
lungs, and the minute ramifications of the extrem
the bronchial vessels; when he also considers, tha
parts of the lungs with which the air comes into con
spread out, would present a surface equal to that of
perficies of the whole body; and lastly, that this v
so exceedingly spongy and porous, that when once i
it is ever after impossible by ordinary mechanical m
expel the air completely; he will easily perceive, n
that a large portion of air must remain for a consi
time in contact with the internal surface of the lungs,
it is liable to lose a portion of its oxigen, but also th
residual quantity of air in the lungs, after the most
attempts at expiration, may be very considerable. I
this circumstance, that we attribute the excess of a
the experiments upon oxigen, and pretty deep inspi
of this gas having been made during 9′20″, the azote
have been in great measure displaced. Admitting
that the air contained in the lungs, before the experi
consisted of 16 oxigen, 84 azote, and at the conclus
the experiment of 94 oxigen, 6 azote, we have

$\frac{84x}{100}$ azote at the beginning,

$\frac{6x}{100}$ azote at the end.

$$\frac{6x}{100} + 100 = \frac{84x}{100},$$

$$110 = \frac{84x}{100} - \frac{6x}{100} \text{ or, } 84x - {}^{\cdot}06x = {}^{\cdot}78x.$$

$$x = \frac{110}{{}^{\cdot}78} \text{ or } 141 \text{ cubic inches;}$$

Th

State of the lungs before the experiment,

Therefore upon this calculation it appears, that 141 cubic nches of gas remained in the lungs after a forcible attempt t expiration; then the state of the lungs before the expe- iment must have been

118·44 azote,
22·56 oxigen.

141

and after.

And after the experiment,

132·54 oxigen,
8·46 azote.

141

Calculation on total Quantities.

Calculation on the whole quantities.

Azote before the experiment	81·50 cubic inches,
——— contained in the lungs	118·44
	199·94
Azote after the experiment,	
——— found by the tests,	191·58
——— contained in the lungs,	8·46
	200·04
Oxigen before the experiment,	3178·50
——— contained in the lungs,	22·56
	3201·06
Oxigen after the experiment,	
Found by the tests,	3001·42
Contained in the lungs,	132·54
	3133·96
Total of oxigen before the experiment,	3201·06
Total of oxigen after the experiment,	3133·96
Difference	67·10

Deficiency of oxigen.

The deficiency noticed in the experiment was 67, supposing that the lungs were brought to the same state after as before the experiment; but granting that this was not the case, and that at the close of the experiment the state of the lungs was 141 + 67 = 208; still our approximation will come within four or five cubic inches, for the azote contained in the sixty-seven missing would be only about four cubic inches. We are aware, that, the temperature of the lungs being 97, while that of the gas was 55°, the 141 cubic inches would occupy a space equal to 154 cubic inches; but this residual quantity must be greater or less according to the exertion made, and also probably according to the state of the muscular fibre at the time.

Seventeenth Experiment.

17th experiment.

The water gasometer was filled to the usual mark upon the scale with oxigen gas prepared from about 9 oz. troy of hyperoxigenised muriate of potash, as in the former experiment; the gas being examined was found to contain as before, 2·5 azote, and 97·5 oxigen, in 100 parts.

Effects on the operator.

The apparatus being found air tight, and all the tubes &c. cleared of atmospheric air by passing oxigen through them, the operator prepared himself for the experiment; but it must be noticed, that he had been rather fatigued during five hours previous to respiring, and had not taken any refreshment; the weather was very warm; his pulse 86; heat under the tongue 98½; he felt no uncomfortable sensation during the process, but experienced a gentle glow and universal perspiration, breathing all the time with great ease, his pulse after the experiment 102, and the heat under the tongue 99°.

Results.

Barom.	Therm. Faht.	Time.	Cub. inches of oxigen gas inspired.	Cub. inches expired.	Deficiency.
30·3	70°	7′ 25″	3420	3362	58.

The quantities of expired gas taken off in each of the mercurial gasometers were as under, in the order in which they were filled.

No. 1····250 cubic inches.	No. 8····296 cubic inches.
2····290	9····256
3····272	10····256
4····238	11····286
5····252	12····257
6····300	13····168
7····241	
	3362

Examination of the expired air.

The 13th gasometer was the whole of the last single and forcible expiration; portions were saved from each of the gasometers, and we first examined the state of No. 1.

100 parts contained 9 carbonic acid,
25 azote,
66 oxigen,

100

The large quantity of azote in this case was a clear proof, that our conjecture upon the residual gas in the lungs was well founded.

We then examined a mixture of No. 2 and 3.

100 parts contained 10·5 carbonic acid,
10 azote,
79·5 oxigen,

100

Here the quantity of azote was diminishing, and the ratio of carbonic acid increasing, so that it appears necessary for the lungs to be cleared of azote, before the increased proportion of carbonic acid can take place.

The 13th or last gasometer was now examined by itself;

100 parts contained 12·5 carbonic acid,
5·5 azote,
82 oxigen.

100

Here the proportion of azote was only 3 per cent more than what existed previously in the gas, and hence we may conclude, that even seven minutes and a half were not sufficient time to remove the azote from the extremities of the bronchia.

We lastly made a mixture of all the gasometers, from 2 to 12 inclusive, and found that 100 parts contained

12 carbonic acid,
6·5 azote,
81·5 oxigen,

100.

Calculation for Carbonic Acid.

Carbonic acid calculated.

100 : 9 : : 250 : 22·50	Carbonic acid gas in No. 1.	22·50
100 : 12·5 : : 186 : 21	ditto No. 13.	21

From 3362 total expired

250 No. 1.
168 No. 13.

Deduct 418

Leaves the mixture 2944 of No. 2 to No. 12.

100 : 12 : : 2944 : 353·28	Carb. acid gas in mixture 2 to 12.	353·28
		396·78

The total quantity of carbonic acid gas emitted was therefore 396·78 cubic inches.

Calculation for Azote.

Azote calculated.

100 : 25 : : 250 : 62·50	Azote in No. 1.	62·50
100 : 5·5 : : 168 : 9·24	—— in No. 13.	9·24
100 : 6·5 : : 2944 : 191·36	—— in mixt. 2 to 12.	191·36
		263·10

The azote expired, beside what might be contained in the lungs at the close of the experiment, was therefore 263·10 cubic inches. Here it is plain, that the operator, at the beginning of this experiment, had not brought his lungs to the same state as in the preceding; or that in consequence of fatigue, and want of refreshment for several hours, the proportion of azote in the lungs might be greater.

Every 100 parts of oxigen, before it was inspired, contained 2·5 azote.

$$100 : 2{\cdot}5 :: 3420 : 85{\cdot}50;$$

Consequently it contained 85·50 cubic inches of azote.

From 263·10
Deduct 85·50 the original azote,

177·60 will be left for the increase of azote.

Then supposing as before, that the quality of the air in the lungs before the experiment was 84 per cent azote, 16 oxigen; and after the experiment 5·5 per cent azote, 94·5 oxigen, as found in the last gasometer, we take

$$\frac{84\,x}{100} \text{ azote at the beginning,}$$

$$\frac{5{\cdot}5\,x}{100} \text{ azote at the end;}$$

$$\frac{5{\cdot}5\,x}{100} + 177{\cdot}60 = \frac{84\,x}{100}$$

$$177{\cdot}60 = \frac{84\,x}{100} - \frac{5{\cdot}5\,x}{100} \text{ or } {\cdot}84x - {\cdot}055\,x = {\cdot}785\,x,$$

$$x = \frac{177{\cdot}60}{{\cdot}785} \text{ or } 226 \text{ cubic inches.}$$

Hence it appears, that previous to the experiment the lungs contained in this instance 226 cubic inches; and if we suppose them to be in the same state after as before the experiment, the quality of the gas in each case will be as follows:

Contents of the Lungs before the Experiment.

Contents of the lungs before:

189·84 cubic inches of azote,
36·16 oxigen,

226

Contents of the Lungs after the Experiment.

and after the experiment.

12·43 cubic inches of azote,
213·57 oxigen,

226

Calculation for Oxigen.

Oxigen calculated.

3420 — 85·50 = 3334·50 original oxigen,
Add 36·16 in the lungs before the experiment.

3370·66 { total quantity of oxigen before the experiment.

After the Experiment.

100 : 66 : : 250 : 165	oxigen in No. 1.	165
100 : 82 : : 168 : 137·76	—— in No. 13.	137·76
100 : 81·5 : : 2944 : 2399·36	—— in mixt. 2 to 12.	2399·36
	—— in carbonic acid	396·78
	—— in lungs after expt.	213·57
		3312·47

3370·66 original oxigen,
3312·47 after experiment,

58·19 deficiency.

Deficiency.

The observed deficiency in this experiment was 58.

The deficiency in this case, and in the former experiment with oxigen, though comparatively small, when contrasted with the quantity of gas respired, is larger than the average with atmospheric air; it seems probable, therefore, that a portion may be detained in the system. It must be remembered, that what we call residual gas is not only that contained in the substance of the lungs, and in its appendages, but also that contained in the fauces and mouth.

Eighteenth Experiment.

18th experiment.

Barom.	Therm.	Time.	Cub. inches of oxigen gas inspired.	Cub. inches expired.	Deficiency.
30·15	70°	8′45″	3130	3060	70.

The operator breathed as usual, after having made a strong effort to exhaust his lungs; his pulse before the ex-

eriment

periment was 84, the thermometer under his tongue 98°: after the experiment his pulse was 96, and the thermometer under his tongue still 98°; the same gentle glow and perspiration was felt as in the other experiments on oxigen; a portion of the gas was saved from each of the mercurial gasometers, and their amounts were as under:

Effects.

Results.

1.	196	7.	258
2.	238	8.	272
3.	284	9.	250
4.	294	10.	304
5.	248	11.	223
6.	280	12.	223
			3060

State of the expired air.

No. 1, tried by itself, contained in 100 parts,

9 carbonic acid,
22 azote,
69 oxigen,

100

No. 12, the last, contained in 100 parts,

12 carbonic acid,
5 azote,
83 oxigen,

100

On account of an accident we cannot give the proportions contained in 2 to 10; but the contents of the first and last gasometers confirm the former experiments, and shows that the proportion of azote continues to diminish, as the experiment proceeds; and also that there is a larger proportion of carbonic acid given off when oxigen is employed, instead of atmospheric air.

Leading facts.

In this recital of experiments, which have occupied a considerable portion of time and attention, we have endeavoured to give a plain statement of facts, from which every one may draw conclusions for himself; we shall here, however, take the liberty of briefly recapitulating the principal of those facts, and submitting what seems to us the most obvious inferences.

No water formed in the lungs.

1. It appears that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxigen consumed, and therefore there is no reason to conjecture, that any water is formed by a union of oxigen and hidrogen in the lungs.

Carbonic acid formed.

2. Atmospheric air once entering the lungs returns charged with from 8 to 8·5 per cent of carbonic acid gas; and when the contacts are repeated almost as frequently as possible, only 10 per cent are emitted.

The 12th and 13th experiments prove, that, when the inspirations and expirations are more rapid than usual, a larger quantity of carbonic acid is emitted in a given time, but the proportion is nearly the same, or about 8 per cent. The proportions of carbonic acid gas, in the first and last portions of a deep inspiration, differ as widely as from 3·5 to 9·5 per cent.

Average proportions.

3. Considering the 11th as a standard experiment, it appears, that a middle sized man, aged about thirty-eight years, and whose pulse is seventy on an average, gives off 302 cubic inches of carbonic acid gas from his lungs in eleven minutes; and supposing the production uniform for twenty-four hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains; the carbon in which is 5363 grains, or rather more than 11 oz. troy. The oxigen consumed in the same time will be equal in volume to the carbonic acid gas; but it is evident, that the quantity of carbonic acid gas, emitted in a given time, must depend very much upon the circumstances under which respiration is performed; and here it may be proper to notice, that all the experiments were made between breakfast and dinner.

Under some circumstances oxigen absorbed.

4. When respiration is attended with distressing circumstances, as in the 14th and 15th experiments, there is reason to conclude, that a portion of oxigen is absorbed; and in the last of these experiments we may remark, that, as the oxigen decreases in quantity, perception gradually ceases, and we may suppose, that life would be completely extinguished on the total abstraction of oxigen.

5. A larger proportion of carbonic acid gas is formed by the human subject from oxigen, than from atmospheric air.

Oxigen gas alone forms most carbonic acid.

6. An easy, natural inspiration is from 16 to 17 cubic inches in the subject of these experiments, who makes about 19 in a minute; this, however, will vary in different individuals; and perhaps we ought to estimate the quantity of carbonic acid gas, given off in perfectly natural respiration, at somewhat less, and most likely at considerably less, than in the statement above, when we consider, that in short inspirations the quantity of air which has reached no farther than the fauces, trachea, &c., bears a much larger proportion to the whole mass respired, than when the inspirations are deep.

7. No hidrogen, or any other gas, appears to be evolved during the process of respiration.

No other gas evolved in respiration.

8. The general average of the deficiency in the total amount of common air inspired, appears to be very small, amounting only to about 6 parts in 1000, and we are inclined to attribute it in great measure to the difficulty in exhausting the lungs as completely after an experiment as before it; the first expiration being made into the open air, the last into the apparatus.

Deficiency of the air very small.

9. The experiments upon oxigen gas prove, that the quantity of air remaining in the lungs and its appendages is very considerable, and that, without a reference to this circumstance, all experiments upon small quantities of gas are liable to inaccuracy.

Quantity retained in the lungs considerable.

Other important conclusions might perhaps be drawn from the facts related in this paper, but having already trespassed largely upon the time of the Society, we shall abstain from any farther remarks, until we bring forward a new series of experiments.

V.

A Letter on Comets, addressed to Mr. Bode, Astronomer Royal at Berlin. Received from the Author.

SIR,

Occasion of the letter.

YOU wished to have in writing the conjectures on the nature of comets, which I had the honour of mentioning to you in conversation a few days ago. At present however they form but the embryo of a system, fully to unfold which would require studies I have not pursued, particularly that of the astronomical history of these bodies, and of the opinions that have been entertained respecting them. I cannot refuse, however, to deliver into your hands these rudiments of ideas, relying on your indulgence both with respect to their want of precision, and to the brevity with which I treat the physical principles, on which they are founded; as these principles are more fully exhibited in different works of mine. But I will at least attempt to explain in a general way the consequences I deduce from these principles with respect to the *luminous* appearances of comets; persuaded, that, if they contain a single seed of truth, your intimate acquaintance with the heavenly bodies will enable you to discern and expand it.

Its subject.

All luminous substances give out light in consequence of chemical decomposition

I shall set out with what the whole of our knowledge of nature appears to me to teach us very clearly respecting the substances, that are capable of becoming *luminous*: this is, that the *light*, which then escapes from them, had entered into their *composition* as an ingredient, and is evolved by chemical *decomposition*.

Light of the Sun from a fluid surrounding it.

In applying this principle to the heavenly bodies, observation has added one circumstance with respect to the Sun, which may be extended by analogy to the *fixed stars*. Dr. Herschel, you know, has discovered, that the *light*, which issues from the *Sun*, does not come directly from its *solid substance*, but from an atmosphere, or some fluid, by which it is surrounded. Thus this grand phenomenon has been brought nearer to us as it were; analogy, though the subjects

Luminous phenomena of

jects differ extremely in magnitude, connecting it with the *luminous* phenomena observed in our atmosphere, such as the *aurora borealis*, *luminous* clouds, and many other luminous meteors. All these are so many *luciferous* vapours, though of different kinds; which, being raised in the atmosphere in their *compound* state, and meeting with something to *decompoſe* them, emit the *light*, that entered into their composition.

our atmosphere.

Proof that the light of the Sun is not from its body.

To determine with more precision what relates to the Sun, we should consider what led Dr. Herschel to the discovery, that its *light* did not issue directly from its *solid* body, but from an atmosphere surrounding it. This was its changeable *spots*; a phenomenon, that had greatly perplexed astronomers and natural philosophers, but which is clearly explained by the discovery of Dr. Herschel, that these spots are the *body* of the Sun itself, *nonluminous*, and seen through transparent parts of its *luminous* envelope. This shows, that, though the *light* issues from a fluid belonging to that kind of atmosphere, which surrounds the Sun, this fluid is not the atmosphere itself, but is simply mixed with it; and this not completely throughout, since there are parts that remain *transparent*, or from which no *light* emanates.

But it is from a permanent lucid covering,

Another consideration determines this phenomena still more precisely. The *disk* of the Sun, as measured by us, is properly that of its *lucid covering*, in the edge of which Dr. Herschel has discerned *gaps*, when any large *spot* arrives there. This *covering* then has a constantly uniform thickness, since we perceive no change in the diameter of the Sun, which is determined by it. Now this is not the character of an *atmosphere*, which in the general acceptation of the term, taken from the atmosphere of the Earth, is considered as formed of one or several *expansible fluids*, the density of which diminishes in proportion as the strata are more remote from their base, so that they extend by imperceptible gradations to an indefinite distance. In fact, when astronomers have spoken of an atmosphere of the Sun, they have ascribed to it a vast extent. Thus, if the *luciferous* fluid, that surrounds the Sun, have such a constant thickness, and be so well defined, that it has hitherto been taken for

not of the nature of an atmosphere,

but rather of a stratum of clouds.

for the body of the Sun itself; it must be a kind of vapour, which, emanating incessantly from the Sun, always rises to the same height in its *atmosphere*, and there forms a stratum as it were of clouds, which is decomposed at a certain height, like the *luminous clouds* that are sometimes formed in our atmosphere. By this *decomposition* the light escapes; and the substances, with which it was combined, must form some kind of dew, that falls back again upon the

Spots in the Sun.

Sun. Now the *transparent* spaces, through which we see the opake body of the Sun, are parts of its atmosphere, into which the *luciferous* vapour does not ascend, or where, when it arrives there, it does not find the ingredients necessary to decompose it into *clouds*. The latter case would be

Openings between our clouds.

analogous to what we see take place in our own atmosphere, when through the intervals of the clouds we discern the blue colour of the transparent air. The matter of clouds is there, since, as I have proved, they are formed by the decomposition of the air itself; but in these spaces the ingredient, which decompose the air, and which apparently rises from the Earth, is not present.

The same theory applicable to other celestial bodies, but with certain modifications.

This theory of the luminous effects of the Sun may be applied directly to the fixed stars; and it may also be extended to other large bodies with some modifications. The atmospheric effects of every one of the bodies dispersed through space depend on its particular *constitution*. To the constitution of the Sun and fixed stars are owing the greatness and constancy of the luminous phenomena, by which they are distinguished. But other bodies, from the difference of their constitutions, may produce phenomena of a similar kind, though much inferior in different degrees, more or less rarely, and under different appearances. Our Earth, though of the lowest order in this respect, maintains its analogy by the *phosphoric* phenomena of its atmosphere; and there may be many intermediate degrees between the highest and this, the lowest.

Comets.

From this last consideration has arisen my conjecture respecting the nature of *comets*; but to establish it demands at least a knowledge of every thing, that has been observed respecting these bodies, which I have not studied. What I have the honour to lay before you therefore, Sir, you will consider only as the rudiments of ideas, which observation and reflection may perhaps hereafter confirm.

Their light is from some chemical decomposition,

I have laid it down as a principle, that the *light* which emanates from phosphoric bodies can arise only from a *decomposition*, which liberates this light from its combination with other ingredients, and which must be induced and influenced by preceding chemical actions on these bodies. This general principle is deduced from the experience of all the luminous phenomena, that are exhibited in our chemical processes, and all that we observe to take place spontaneously on our globe: and this is all we can refer to the other large bodies in the decreasing scale of these effects, from the Sun and fixed stars, the light of which is so intense and permanent, to the phosphoric phenomena of our atmosphere. Thus we can determine nothing with respect to the constitution of these bodies, the nature of the chemical processes that extricate light from them, the periods in which these effects must be reproduced, or lastly their different appearances when they are produced. Here then we find many causes of uncertainty in the application of this principle to the phenomena of *comets*; and accordingly every thing I have to say on the subject must be vague and hypothetical.

of the nature of which we know nothing.

Small bodies revolve round the Sun occasionally visible

I conceive, that a great number of bodies revolve round the Sun at different distances, and with different motions; which from the smallness of their size, and more especially from the nature of their surface, cannot be rendered visible to us by reflection of the solar rays; but which at certain times, or under certain circumstances, are capable of producing *luciferous vapours*.

Their luminousness affected by their nearness to the Sun.

Phenomena of the Bolognian phosphorus.

Let us suppose, that a certain degree of proximity to the Sun is a circumstance determining either the production or the decomposition of these vapours; in other words, that the intensity of its rays is the chemical cause, that induces the luminous effects. I have been led to this idea by a phenomenon observed in our experiments, which displays some analogy with the supposition here made: certain bodies recently calcined, or calcined anew, such as oyster shells and the Bononian stone, after having been some time exposed to the *rays of the Sun*, are *luminous* in the dark.

Euler imagined, that he could draw from this phenomenon an argument against Newton's theory of the *emission of light*, and in favour of his theory of vibrations: conceiving, that the effect of the rays of the Sun on these bodies was to produce in them *vibrations*, which continued for some time. On the contrary I deduce from it an argument militating directly against his theory: for he supposes, that the different *colours* of light are different kinds of *vibrations*; so that when these bodies are exposed to rays of certain *colours*, which can produce vibrations of their own kind only, the light afterward emitted by these bodies in the dark must be of the same *colour*. Now from the experiments of Mr. Wilson, which I myself have seen, the light emitted by a calcined oyster shell is always the same, whatever was the colour of the ray, to which it had been exposed. These oyster shells acquire different properties in this respect, according to the degree of calcination, or some other unknown circumstance. I have seen them emit red, green, and yellow light: but they never shine again, each with its own colour, except from the immediate action of the solar rays; and each single coloured ray causes them to emit the same colour, but more faintly, the ray of their *own colour* having no advantage over the rest. Hence I infer, that the rays of the Sun produce a *decomposition* in these calcined substances, in consequence of which they give out the *light*, that entered into their composition. The decomposition is thus accelerated, and goes on for some time in the dark; but without this it would go on too slowly, for the *light* that is evolved to be perceptible; though it does go on, for the calcined oyster shells do not long retain this property.

Light not a system of vibrations.

Comets may differ from each other, and from themselves at different times.

Though a greater proximity of the Sun may thus be the cause of a more rapid decomposition of the luciferous vapours that arise from comets, this approximation cannot produce the same effect either on different comets, or on the same comet on its return to its perihelion, unless the production of these vapours go on constantly with the same degree of intensity; which there is no reason to suppose: for there may be a great difference in this respect between different comets, and in the same comet at different times.

If these vapours then rise to a considerable height above a comet, before they are phosphorescently decomposed; not having, like those of the Sun, a fixed limit of ascent; when the comet approaches its perihelion these vapours will become luminous, and the comet may appear as a lucid nebulosity merely, with an irregular boundary, and without our being able to distinguish any determinate disk: and when such a disk is observed, it is probably owing, as in the case of the Sun, to the regularity of the lucid vapour, without the body of the comet, which is surrounded by vapours, being at all visible.

Comets without a nucleus and with.

If such be the cause of the light emitted by comets; that is, if it be owing to luciferous vapours, which are decomposed; and if the rays of the Sun produce or accelerate this decomposition, when they approach their perihelion; their light may be at first faint, then increase more than in proportion to their approximation, and decrease more rapidly than in proportion to their recession. If then their apparent magnitude enter into the calculation of their orbit, may not this be a cause of errour?

May not their apparent magnitudes lead to errours in calculation.

Farther, as the *light* of comets appears to be certainly *phosphoric*, and not reflected; as *phosphoric* effects unquestionably depend on chemical operations proceeding from the composition of bodies: and as these operations in comets do not appear to follow as regular a course as in the Sun; may not this be the reason, that we are so little capable of recognizing comets on their return, though their elements have been accurately determined, and we have reason to expect their return at stated periods? for they may actually return at these periods without being known, on account of their having a very different appearance; or even without being visible, as the circumstances, which caused them to emit light at the time when they were observed, may not exist. This would render unnecessary the supposition, which some astronomers have adopted, that in their very eccentric paths they meet with bodies, the vicinity of which obliges them to alter their course.

A comet may not be known at its return from the difference of its appearance, so that a perturbation of its orbit is not necessary to account for it.

One of the most remarkable phenomena of *comets*, the *tail* which they commonly have, excites us strongly to inquire into its cause: but not to give up the reins too much

Tails of comets.

to imagination, we must look for some circumstances in the phenomenon itself, and in analogy, to limit our field of inquiry. One circumstance in the phenomenon itself, which tends to answer this purpose, is, that the *tail* of a *comet* is always on the side farthest from the Sun: and analogous to this on our globe we find the *luminous corruscations*, that frequently accompany the *aurora borealis*, likewise take place in our *night*, that is opposite to the Sun. Farther, sometimes we see in the mass of atmospheric fluids, all of which gravitate toward the Earth, a fluid sometimes formed, which, from the prodigious rapidity of its motion in a right line, experiences no perceptible deflection from the action of gravity; which thus, if it dart vertically upward, may soon get out of the sphere of gravitation toward our globe, and which is *luminous* in its course. I mean the *electric fluid*; for this, though little deflected by gravitation in its rapid course, is subjected to another law, which retains it in the atmosphere. This law is its strong tendency to unite with the particles of the *air*, which occasions the *lightning* to disappear in its horizontal movements, after the zigzags it has been forced to make by the resistance of the *air*; zigzags which are not perceptible in the corruscations of the aurora borealis. Suppose then, that a fluid in all other respects similar to the *electric* is formed on that side of a comet which is turned from the Sun; a fluid so rapid in its rectilinear motion, that it soon gets out of the sphere of gravity of that body, without being detained by its atmosphere, and which is rendered *luminous* by its decomposition on its course: thus we shall have the phenomenon of the *tail*, with a circumstance characteristic of these causes, namely the *curvature* of these tails, the *convexity* of which is turned toward the side to which they are moving; the cause of which appears to me to be as follows.

Aurora borealis.

Electric fluid.

Lightning.

Tails of comets a similar fluid.

The particles of the fluid of the tail, as they are detached from the comet, possess the same *projectile* movement with it, and in the same direction. Accordingly they must continue to follow it. But if they extend very far, that is to say, if the tail become very long; the particles that proceed the farthest, continuing to move with the same velocity but in a larger orbit, must have a less *angular* movement. This must

Cause of their curvature.

must produce such a *curvature*, not to mention some degree of *resistance*, which these particles may experience in *space*, though with respect to large bodies it is imperceptible. Finally, the *tail* will extend so much farther to our eyes, in proportion as the production of this fluid is more abundant, and its *decomposition* more slow; both which circumstances may vary, not only in different comets, but in the same comet at its different returns. Combining these different considerations, therefore, may it not happen, that a comet observed with a very long tail, and of considerable apparent diameter, from the greater spread of its envelope of *luciferous vapours* to a determinate distance from its body, shall return with very little tail, and a much less apparent diameter; that its light even shall be weaker; and that thus it shall not be known for the same as was expected from calculation, though in reality it is so?

Conclusion.

Here let me stop. He who coasts along the shore by the help only of the lead and the log, without a compass and without a quadrant, must not set up for a geographer, but confine himself to hints. In conjectures like the present, the analogies on which they are founded can furnish only germes of a theory: time alone can show by repeated observations, whether they be capable of becoming fruitful; but at least they afford matter for reflection and inquiry, and the first ideas I communicated to you have already been corrected and extended by your remarks. At your desire I now deliver them to you, such as they are, in testimony of my respect, &c.

Berlin, 2 *June*, 1799.

XII.

An Example of the Utility of a Series in finding a Fluent. By a Correspondent.

To Mr. NICHOLSON,

SIR,

Difficulty of direct solutions.

IT sometimes happens, that great mathematicians give themselves no inconsiderable pains to untie a knot, which they might cut with the greatest ease. As an instance of this, I beg to offer you a determination of a fluent, which seems to have been considered as presenting some difficulties scarcely to be overcome.

The equation to be resolved is this; $\int xy\dot{x}\sqrt{(\dot{x}^2+\dot{y}^2)} = mx\dot{y}$; hence, by squaring both sides, we have $[(\int xy\dot{x})^2 - m^2x^2]\dot{y}^2 + (\int xy\dot{x})^2\dot{x}^2 = 0$, and $[(\int xy\dot{x})^2 - m^2x^2]\frac{\dot{y}^2}{\dot{x}^2} + (\int xy\dot{x})^2 = 0$. Put $y = a + bx^2 + cx^4 + dx^6 + \ldots$, then $\int xy\dot{x} = \frac{1}{2}ax^2 + \frac{1}{4}bx^4 + \frac{1}{6}cx^6 + \ldots$; of which the square may be called $Ax^4 + Bx^6 + Cx^8 + \ldots$: again, $\frac{\dot{y}}{\dot{x}} = 2bx + 4cx^3 + 6dx^5 + \ldots$, the square of which may be called $\alpha x^2 + \beta x^4 + \gamma x^6 + \ldots$; then, by multiplication and substitution, our equation becomes $(A - \alpha m^2)x^4 + (A\alpha + B - \beta m^2)x^6 + (A\beta + B\alpha + C - \gamma m^2)x^8 + \ldots = 0$; whence, by the general law of infinite series, $A = \alpha m^2$, $A\alpha + B = \beta m^2$, $A\beta + B\alpha + C = \gamma m^2$, …, and $\alpha = \frac{A}{m^2}$, $\beta = \frac{A\alpha + B}{m^2}$, and so forth; but, by actual involution, we find $A = \frac{1}{4}a^2$, $B = \frac{1}{4}ab$, $C = \frac{1}{6}ac + \frac{1}{16}b^2$, …, and again $\alpha = 4b^2$, $\beta = 16bc$, $\gamma = 24bd + 16c^2$, …, whence $b = \sqrt{\frac{\alpha}{4}}$, $c = \frac{\beta}{16b}$, $d = \frac{\gamma - 16cc}{24}$, $e = \frac{\delta - 48cd}{32}$, and so forth; and finally, by reduction, we have $b = \frac{a}{4m}$, $c = b^3 + \frac{b}{16m}$, $d = 3b^5 + \frac{5b^3}{18m} + \frac{b}{576m^2}$, and $e = 5b^7 + \frac{35b^5}{32m} + \frac{41b^3}{1152m^2} + \frac{b}{36864m^3}$; and in the case where a becomes extremely small, so that its higher powers may be neglected, $c = \frac{b}{4^2m}$, $d = \frac{c}{6^2m}$, $e = \frac{d}{8^2m}$, and $f = \frac{e}{10^2m}$. If, in particular cases, beside the general equation, we have a determination of the value of $\int xy\dot{x}$, for instance $\int xy\dot{x} = nx$, we may obtain an equation between x and a, whence either of them may be found from the other; that is, $\frac{1}{2}ax + \frac{1}{4}bx^3 + \frac{1}{6}cx^5 + \ldots = n$: hence, if x becomes very small, $a = \frac{2n}{x}$; and if a is evanescent, $\frac{a}{2}\left(x + \frac{x^3}{2\times 2^2 m} + \frac{x^5}{3.2^2.4^2m^2} + \frac{x^7}{4.2^2.4^2.6^2m^3} + \ldots\right) = n$.

For

Examples.

For example; let $m = \frac{1}{200}$, and $n = \frac{3}{800}$; and let a be first $\frac{1}{200}$: then b will become $\frac{1}{4}$, $c = 3{\cdot}14$, $d = 20{\cdot}5$, and $e = 76{\cdot}7$; and supposing $x = {\cdot}3$, the equation for n will become $\frac{3{\cdot}6}{800}$ by the correct series, and $\frac{3{\cdot}44}{800}$ by the approximation: then in order to reduce n to its true value, we may may make $x\,y\,x' = (n\,x)' = x\,n'$, very nearly, and $x' = \frac{n'}{y}$; but y is found, in this case, ·074, and $x' = \frac{{\cdot}6}{800 \times {\cdot}074} =$ ·01014, whence x is accurately ·2898: or, x being ·3, a will be nearly $\left(\frac{3}{3{\cdot}44}\right) \times {\cdot}005 = {\cdot}0044$, by the approximation. In the second place, let a be $\frac{1}{20}$: then b will be $\frac{1}{4}$, $c = 46{\cdot}875$, $d = 1237$, and $e = 47200$; and suppose $x = \frac{1}{3}$; then n becomes ·00382, $n' = {\cdot}00007$, and $\dot{x}$ corrected, ·1104. For a third example, if x be $\frac{1}{3}$, we may find an approximate value of a by the second series, which is ·0156; and this being necessarily somewhat too large, we may assume $a =$ ·015, whence n is found ·00371, and n' being ·00004, a' will be nearly $\frac{a}{n} n'$, or ·00016, and a, corrected, ·01516. In the first and third examples, the calculations of an eminent mathematician on the continent, deduced nearly from the same data, have given for a, instead of ·0044 and ·0152, ·0038 and ·0136; while some very accurate experiments had long before made the results ·005 and ·015.

Other cases.

If we wished to investigate the properties of the returning branch of the curve, of which x and y are the coordinates, it would be necessary to apply a proper correction to the fluent $\int x\,y\,\dot{x}$; another form of a similar curve might also be obtained by substituting $p - y$ for y; and when x becomes infinite, we find, by a different mode of calculation, beginning from the other end of the curve, $-x = \sqrt{(p^2 - y^2)} + \frac{p}{4}\,\mathrm{H.L.}\,\frac{p - \sqrt{(pp - yy)}}{p + \sqrt{(pp - yy)}}$.

I am, Sir,

Your very obedient servant,

13 *Feb.* 1809. E. F. G. H.

VII.

VII.

An Account of the Method of cultivating the American Cranberry, Vaccinium Macrocarpum, *at Spring Grove. By the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c*[*].

Cranberry cultivated with success.

THE American cranberry, the *vaccinium macrocarpum*, has for some years been cultivated with success at *Spring Grove*, and as the fruit of it is now become an object of some importance in the economy of the family, a short account of the management of this unimproved plant will it is to be hoped prove acceptable to the Members of this useful Society, and not uninteresting to the Public at large.

Supply from water.

For the better understanding the intended communication it is necessary to premise, that a spring rises in a small grove within the precincts of *Spring Grove*, which is no doubt the origin of the name; this spring is carried in leaden pipes into the house, to which it affords an ample supply; the waste water is suffered to run through a small basin and a pond in the pleasure ground, before it escapes to *Smallberry Green*; to this constant supply of fresh water, though it is very small, the great luxuriancy, with which water plants of all kinds suitable to this climate succeed in the pond, is no doubt in some degree to be attributed.

Artificial island.

In the middle of the basin, a small island has been formed, by supporting a box of oak upon posts driven into the bottom; in the centre of this pond, the waste water which used before to issue through a fountain, is suffered to flow in the form of a spring, which rises into a large shell of the *chama gigas*, perforated for the purpose, imitates very well a natural spring, and gives in hot weather an appearance of freshness and coolness, very pleasant to those who walk in the garden.

The oak box, which constitutes this artificial island, is circular, 22 feet in diameter, and 13 inches deep; the bottom is 5 inches under the surface of the water, and bored through with many holes; on this a layer of stones and rubbish was first placed, and upon that a covering of bog earth, brought from *Hounslow Heath*, which together are 5 inches below and 7 inches above the surface of the water of the basin; in this bed of black mould, a variety of curious bog plants were placed about seven years ago, which flourished in an unusual degree, among these was the *vaccinium*, which flowered and ripened its fruit the first year.

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[*] Trans. of the Horticultural Society, vol. I, p. 75.

Bog plants.

Cranberry.

Runners.

In the autumn of the second year it again produced a plentiful crop, and soon after began to send out runners somewhat resembling those of a *strawberry*, but longer, and rather less inclined to take root while young; they did however take root in the winter, and early in the spring threw out upright branches 10 inches and a foot long, on which the flowers and fruit were chiefly placed; the produce was this year gathered, and found to be high flavoured berries, very superior to those imported, which have in general been gathered unripe, and have become vapid and almost tasteless by long soaking in the water, in which they are packed for carriage.

Fruit much superior to that imported.

It was now determined to consider the *American cranberry* as an article of kitchen garden culture, and to give up the whole of the island to it, which in a few years it entirely covered by its own runners, without any fresh plants being put in, and this bed, with the addition of some hanging boxes receding from the centre to the sides, produced, in the year 1806, 23 bottles of very fine *cranberries.*

Cultivated for the kitchen.

In the year 1805, a bed was made on the side of the pond 20 feet long and 5½ feet broad, by a few stakes driven into the bottom parallel to the side, and lined with old boards; the bottom of this was filled up with stones and rubbish, and on these a bed of black mould, 3 inches above and 7 inches below the usual surface of the water, was laid: this was planted with *cranberry* plants, many of them having been rooted in a hot bed, in which they throve most vigorously. In this autumn, 1807, the bed produced a crop, which, added to that of the island, afforded a supply for the family of 5 dozen bottles of cranberries, beside a small basket reserved for present use. The total contents of the two cranberry beds is 326 square feet; the quantity of land employed

Extended to a bed by the side of the pond.

tion of a gardener's proceedings: it origi
a fortunate accident, the history of which,
give an adequate idea of the method now p
the same time bear testimony in favour of t
more benefit has been derived in the advanc
cultural knowledge, by pursuing the hin
continually gives, than from the effects of ab
and original invention.

The crop not liable to injury.

It is remarkable, that, during the seven y
berries have been cultivated at *Spring G*
stance has arisen, from the variety of seaso
or any other circumstance, that has diminish
of a full crop; the flowers have issued out o
abundance, in their due season, and fewer of
abortive, than in general is the case in oth
fruit has gradually swelled and duly ripene
subject to the attack of any vermin, or to
kind from the excesses of heat or cold, or
wetness or of drought.

VIII.

On the Existence of Animal Matter in Min
From Parkinson's Organic Rema

seen, that the colour does not exist in the intermediate calcareous matter, or in that which has been introduced into the cavities of the tubes, but in the substance of the coral, forming the sides of the tubes.

This some unknown modification of animal matter.

It appears, from the experiments of Mr. Hatchett, that the colouring matter of the organ-pipe coral is similar to that of the red coral, *(gorgonia nobilis)* being some unknown modification of animal matter. The red colour of both these substances having been gradually destroyed, during the action of the diluted nitric acid, as the solution of the calcareous substance advanced; and could not afterward by any means be restored: nor could any colouring principle whatever be detected by the reagents usually employed*.

Solution in milk,

We also learn from the Count de Marsilli, that by digesting red coral, with or without its membranaceous tunic, in milk, over a slow fire, the colouring matter was dissolved by the milk, which became thereby of a pink colour; while the coral became first of a saffron, then of an ash colour, and at last of a livid white. The same effects resulted from its digestion in heated wax. A similar deprivation of colour is also found to take place in those species of coral, which, having been broken, have fallen into, and have remained for some time in the mud at the bottom of the sea†.

in hot wax, and in mud of the sea.

Thus most fossils deprived of it.

As the colouring matter of these corals is capable of being thus removed by digestion, it is not to be wondered at, that, in general, in the fossil specimens, which must, in most instances, have been exposed to long continued maceration in water, little or none of the original colour remains.

Separate fossils too siliceous for such an examination.

From the weight and other physical characters, as well as from exposure to chemical agents, I found, that every fossil of this species in my collection, not imbedded in stone, contained too much siliceous matter, to admit, by the agency of an acid, an examination into the change, which had taken place in the original constituent parts of the coral.

Indeed, considering that in the experiments of Mr Hatchett even the recent tubipore lost its colour, and only de-

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* Philosophical Transactions, for 1800.

† Histoire Physique de la Mer, par Louis Ferdinand Comte de Marsilli.

monstrated

and animal membrane demonstrated.

I was much pleased to observe the membran
appear, depending from the marble, in l
elastic membranes. Many of these most u
tained a very deep red colour, and appear
and distinct manner, although not absolute
form of the tubipore.

Another instance of this.

Another curious specimen of marble exhi
stance of animal membrane preserved for ag
tion by being enchased in stone. This
brownish red colour, much darker in its i
than in the corallites themselves, and is susc
polish. The madrepore, which has entered
tion of it, appears to have been of the smal
branches being about the size of a goosequ

Its colouring matter.

The diffusion of the red colour through t
quires some little attention. It extends thr
of the mass, and appears to have percolat
external part of the coral into its internal su
a manner that no gross substance has entere
matter appearing to have been here deposite
tion. That the colour has not been derive
itself, may safely be inferred: there appear
two modes by which this colour may hav
Either the decomposed coral might have be
a matrix, of which the oxide of iron form

the mass, were previous to the perfecting of the lapidifying process.

Nitrous acid detected oxide of iron,

On subjecting this marble to the action of diluted nitrous acid, its decomposition took place very speedily. A considerable quantity of carbonic acid gas was separated, the calcareous part was dissolved, and a red substance, an oxide of iron, gradually sunk to the bottom. While the decomposition was proceeding, a substance was detected, the presence of which, at least in so obvious a state, was not expected. As the separation of the other parts took place, ragged flocculent pieces of apparently a membraneous substance were left, adhering to those parts where the coralline substance had been observable. These, on the least agitation of the fluid, were seen to wave to and fro, and on the motion being increased fell off, and soon reached the bottom of the vessel from the weight of the solid matters which were attached to them.

and exposed a membrane.

Marble from Kilkenny.

In the specimens of marble from Kilkenny, the remains of a coral of this kind, but of a larger species, are very evident. The ground of the marble is of a deep black, but the part of the marble possessed by the coral is of a very light gray. In fineness of grain and in susceptibility of polish it appears to equal any marble. From this circumstance, and from the considerable difference in the colour of the madrepore and of its matrix, the structure of the former becomes very conspicuous, and the astonishing labours of its original inhabitant are very easily traced. In one part, the converging perpendicular plates, displayed by a horizontal section of the madrepore, are discovered; while in another part a longitudinal section has not only shown numerous horizontal plates; but also yields a fair view of the beautiful reticular texture of the coral, resulting from the frequent intersections of the perpendicular by the transverse lamellæ.

Appears to have undergone two petrifying processes.

The very considerable difference of colour in the ground of this marble and in the animal part is particularly deserving of attention. As in the former specimen the regular diffusion of colour through the whole mass appeared to authorize the conjecture, that the colouring matter was introduced previous to the coral having undergone its lapideous

Breccias.

considered, that in several marbles, indeed i
marbles, this twofold lapideous impregnatio
rily be admitted. These are composed of fi
rious marbles, which, after having been fo
strata, have been broken into small pieces,
become agglutinated into a compact mass,
of a fluid, which, from its saturation with t
lime, has possessed the required lapidific
it has exerted during its interposition betwee
fragments.

The marble digested in dilute muriatic acid.

No membrane appeared,

but an animal charcoal.

A piece of this marble was suspended i
containing diluted muriatic acid, and was sp
with effervescence. During the decompositi
of marble, not the smallest filament of
substance became detached; but, on the cont
forming surface was as perfectly clean and
had been a piece of primitive limestone: th
from which the marble derived its colour fall
tom of the vessel, during the solution of the
powder being dried was projected on melted
mediately produced deflagration: a circun
with the form of the coral having been visil
ble, shows the curious fact, that a part of
matter of the marble was an animal charcoal

ached, and slowly sunk to the bottom of the glass. The smallness of the pieces of membrane might be here probably accounted for by the structure of the madreporite which they composed; since from the multitude of pores with which it is pierced, the membrane must suffer such frequent inflexion, as would give very little reason to expect, that, in a corroded preparation of even the recent madrepore, any thing like the form of the madrepore could be preserved. The circumstance of these membranous flocculi appearing to possess a greater degree of gravity than belongs to animal membrane in general may be accounted for, by considering, that they may yet retain some particles of earth in some of their cavities, which are defended from the action of the acid, by being completely enclosed in the membrane.

A very perfect membrane obtained from a spongite.

A small, light coloured, calcareous spongite, or alcyonite, possessing somewhat of a conical form and a rugose surface, and exhibiting, when examined with a lens, evident marks of an original spongeous substance, was suspended in water slightly acidulated with muriatic acid. As the acid acted on the carbonate of lime, the membraneous part of the sponge began to appear; and being liberated, extended itself beyond the remaining solid mass in tolerably coherent flexible flocculi. When it had been submitted so long to the action of the acid, that the carbonate of lime was nearly removed, a dark brown reticulate mass was left, bearing the general form of the fossil, and manifesting its original spongeous reticulated texture. So coherent was the mass, as even to bear its removal, by pouring into another glass, without suffering any material injury to its form. It was however at last broken, upon pouring additional water into the phial which contained it. Another specimen, as similar in its form and size as could be found, was then submitted to the action of the acid for the purpose of obtaining a correct sketch of the remaining membrane, and with equal success.

IX.

IX.

Observations on the different Species of Dahlia, *and the best Method of cultivating them in* Great Britain. *By* R. A. SALISBURY, *Esq. F. R. S**.

Dahlias.

NO flowers, which have been lately introduced into the gardens of this island, are more showy than the *dahlias*; and they possess the additional merit of being produced at a season, when most others are decaying; nevertheless, it will appear in the subsequent pages, that by a little management these plants may be made to blossom at a much earlier period; and that in vallies, or low situations, where our autumnal frosts frequently cut them off early in *October*, it is the only method of obtaining their flowers at all. I am more emboldened to offer these results of my own experience to the Horticultural Society, as they have turned out very different to what was expected from the hints thrown out upon this very subject by one of the first gardeners in the world.

History of them.

First mentioned by Hernandez.

The earliest account I am able to trace of these plants, which are all natives of *Mexico*, is in Hernandez' History of that country, published in 1651, where two species are figured. He says that the first grows in the mountains of *Quauhnahuac*, and is called *acocotli* by the inhabitants; that it has leaves composed of five leaflets, some of which are sinuated, slender peduncles, with pale-red stellated flowers; that the roots are tuberous, strong and bitter in taste, and, according to the fashionable jargon of his time, hot and dry in the third degree; that an ounce in weight, taken internally, is a powerful medicine, alleviating pains in the bowels, expelling flatulence, increasing the urinary discharge, promoting sweat, strengthening cold languid stomachs, excellent against the colic, resolving obstructions, and dissipating tumours if externally applied. This is clearly the pale red variety of *dahlia sambucifolia*. The second he calls *acocotli ligustici facie*, but gives no description of it: the figure however, though destitute of flowers, leaves no doubt, that it is the species called *dahlia bidentifolia* in *Paradisus Londinensis*, and from the size of its foliage most probably the orange coloured variety.

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* Trans. of the Horticultural Society, vol. I, p. 84.

T. Menonville.

Mr. Thiery Menonville, in the interesting detail of his journey to *Guaxaca*, published in 1787, is the next author, who to the best of my knowledge has noticed any species of *dahlia*. It is well known, that this botanist was employed by the French Minister, to steal the *cochineal* insect from the Spaniards. In this dangerous mission, he tells us, that having entered one of the gardens in the suburbs of that city, adjoining to a plantation of *nopals*, upon which the insect feeds, he was struck with the beauty "*d'une as-" tere violette et double, aussi grande que celles de France, " mais produite par un arbuste tres semblable pour les feuilles " pinnées à notre sureau.*" From the violet colour of the flower, I am inclined to think, that this is the species which I have called *dahlia sphondyliifolia*.

Cavanilles.

The third author, who has written upon these plants, is the late Abbé Cavanilles. From a semidouble variety of *dahlia sambucifolia*, which flowered at *Madrid* in *October* 1790, he in the first volume of his *Icones*, published in the following *January*, first defined the characters of the genus scientifically, naming it in honour of Andrew Dahl, a Swedish botanist, with the specific title of *pinnata*. Afterward, in the third volume of the same work, he makes us acquainted with two more species; his *rosea*, which from this ambiguous title has been confounded both here and at *Paris* with his first; and his *coccinea*, no less absurdly so denominated, its ligulated florets varying from yellow to orange, but never assuming a scarlet tint. My reasons for adopting his generic, but none of his specific names, will be given hereafter, and are conformable to the usage of Linne in those classical works *Flora Lapponica* and *Hortus Cliffortianus*.

Name.

Thouin.

These three *dahlia's* having been sent to *Paris* from *Madrid* by Cavanilles in 1802, a very ample memoir with coloured figures was published two years afterward by Monsieur Thouin, in that celebrated national work, the *Annales du Museum d'Histoire Naturelle*, and he makes the fourth

writer

Description of the plant.

writer upon them. We there learn, that they are perennials, losing their stems at the approach of winter, which do not push forth again till late in spring; that their roots consist of fleshy tubers disposed like those of the *asphodel*, though less numerous; that on their arrival they were planted in large pots of substantial earth, and protected from frost under a frame; that the stems grew little till the great heats of summer commenced, when they lengthened rapidly, and flowered in the end of autumn. Monsieur Thouin then describes the first, his *dahlia rose*, as attaining seven feet in height; leaves opposite, composed of from 5 to 9 leaflets; flowers about the size of a *China aster*; ligulated florets commonly 8 in number, pale red inclining to flesh colour; of which all the earlier flowers ripened seeds. The second, his *dahlia ponceau*, was only four feet high; stem slender, covered with a fine meal; leaves doubly pinnated, and pale green; flowers smaller than in the two other species; ligulated florets from 8 to 9, red orange colour; this flowered later, and did not ripen seeds. The third, *dahlia pourpre*, he erroneously supposes to be Cavanilles' *dahlia pinnata*, and thinks it greatly superior in beauty to both the others; the roots of this he observes are covered with a violet coloured cuticle; stems about five feet high; leaves often produced in threes; flowers semidouble; ligulated florets of a rich violet purple, approaching that of the *pansy*, or still more like the fruit of the *prune de Monsieur*, which on their inner surface reflect the light variously (*chatoyante*) like a shot silk; it flowered the latest, and only ripened very few seeds.

His mode of cultivation.

After paying some handsome compliments to Cavanilles for sending, and to Dr. Thibaud for bringing the roots from *Madrid*, this candid and judicious gardener proceeds to state what, he conceives, will be the properest mode of treating these plants. He remarks, that, being newly arrived, with all the original habits contracted in their native climate, it will only be after a lapse of years, that their culture can be thoroughly understood; and that if he anticipates any directions on this head, it is rather to excite the attention of others to the subject, than lay down fixed and positive rules for their conduct. From the magnitude of the roots, the abundant foliage, and rapid expenditure of sap in these plants, he concludes, that a strong but very rich soil, nearly such as *orange* trees delight in, will be most suitable for them, with plenty of water in dry weather. Being ignorant of their particular locality in *Mexico*, he doubts whether they will live through winter at *Paris* in the open ground, giving his opinion in the negative, for the following reasons: 1*st*, herbaceous plants so tall and tender are seldom met with in high mountains, the dominions of winds, snows, and storms; 2*dly*, these plants, when exposed to a temperature of 7 or 8 degrees below zero, turn yellow and sick; 3*dly*, they are late in beginning to vegetate, and require a long protracted autumn to expand their flowers: 4*thly*, their roots had been already killed at *Paris*, by a frost of five degrees in one night. Notwithstanding this unfavourable statement, Monsieur Thouin does not despair of being able in time to change their habits, and acclimate them in *France*: to this end, he proposes forwarding them in spring with a little artificial heat, and wisely remarks, that our days in summer being longer than in *Mexico*, a sufficient maximum of heat to bring their flowers and seeds to perfection may thus be obtained; that thus *barley*, which in the north of *France* requires six months to ripen it, in *Russia* is often perfectly matured in forty days. He then brings instances of two plants from the same country, the *marvel* of *Peru*, and *long-flowered marvel* of *Peru*, which, though very tender when first introduced, are now become more hardy, the former especially often springing up in their parterres from self-sown seeds. Lastly, he informs us, that all the *dahlias* may be increased by seeds, dividing their roots, or even by cuttings of their stems, though that part is annual; but seedling plants, he remarks, do not flower the first year, and the memoir concludes with some general remarks on the beauty, and ornament, which they will add to our borders or conservatories, in autumn.

Longer days equivalent to greater heat in vegetation.

Mode of propagation.

The fifth author upon this genus is Professor Willdenouw, who in his *Species Plantarum* most unwarrantably changes its name, under the pretext that another *dahlia* was already established in *dioecia!* This is so far from being true, that the description of the *Cape* plant he alludes to,

Willdenouw.

by

name of *dahlia*, much inconvenience will ens
if the other be adopted: moreover, it would
Cavanilles, who is dead and gone.

I must now venture to give some account of
tion of the *dahlias* into our own island; when
how rapidly we have improved upon the Fren
treating them; and as they have already not
a number of varieties with us, but each speci
somewhat different management, I shall offer
tions, as I hope will not be found quite useless res

The first species, *dahlia sambucifolia*, was in
1st species. Introduction into Britain. this country by the Right Honourable Lady
sent the seeds from *Madrid* in *May*, 1804, wh
duced several varieties of different colours.
Specific name. son, I have rejected both the specific names *ros*
rea, for one that is applicable to both of them
Monsieur Thouin observes, is equally inadmis
many of its leaves only consist of three le
greater number of the leaves of the 2d speci
pinnated, it has already occasioned muc
Though the seeds arrived so late in this count
them flowered the succeeding autumn at *H*
and the variety ζ with deep purple flowers wa
pretty well figured in the *Botanist's Reposi*

plunged to their rims in a bed of dung, which had nearly lost its heat, having been made two months. A dozen plants soon came up; and on the first of *June*, being about 8 inches high, as well as very stiff, from throwing down the glasses in the day time, were transplanted into separate pots of 2½ inches diameter. In these they continued three weeks, when two of the strongest were removed without breaking any of their fibres into large pots of very rich mould, with the intention of following Monsieur Thouin's directions minutely; five of them into pots one size larger, of very rich mould, and five of them into pots one size larger, of poor sandy mould. All these plants were twice more transplanted into somewhat larger pots before the 10th of *August*, by which time the two largest were 4 feet high, and the others not much shorter, though less branched. They were now all removed from the hot-bed frame, having been exposed to the open air both night and day the last month; the two largest into a border of rich earth, but the rest plunged as they stood in the pots, in various parts of the garden, near the walls, but only in west and east aspects, that to the south being entirely filled with other plants. Their stems and branches, as they advanced, were carefully secured from being broken by the winds, and they were supplied with water, whenever their leaves flagged. They all grew rapidly in *August* and *September*, but I despaired of seeing any flowers till the middle of the latter month, when almost every branch terminated in a flower, the first of which opened the 7th of *October*. Soon after others came out; but what is well worthy of attention, the two largest plants, which had been nourished the most luxuriously, though placed in the warmest corner, were the latest in showing flowers. One of these, which had attained to twelve feet in height, did not expand its first flower till the 29th of *October*, producing however a plentiful succession till the beginning of *December*; in the first week of which a violent storm of wind and rain nearly put a stop to its vegetation. All the plants ripened seeds more or less, and were suffered to remain in the ground with their decaying stems uncut, till a frost came, which was severe enough to freeze the borders an inch deep. The morning after, those

Flowering.

those which had been plunged in their pots were taken up and removed into the greenhouse, behind other plants: the two in the ground, after cutting down their stems to about a foot and a half in length, and removing the frozen crust of earth, were protected with a covering of moss and fern about six inches thick.

Management the second year

In 1807, the greenhouse plants were removed into the open air so early as the 27th of *April*; and the *dahlias* at the same time just beginning to push were turned out of the pots, and planted in very different parts of the garden, as well as in very different soils. Having observed the preceding year, that those which had been confined in the smallest pots and poorest earth not only flowered the earliest, but made to a gardener's eye the handsomest plants, being only from 5 to 6 feet high, with scarcely any branches, and panicles of from 7 to 13 flowers; I ordered some of them to be placed in pure gravel from which all the larger stones had been screened, others in a dry seam of sand which crossed the garden, and others again in rich earth; they were all supplied with water however during the dry part of the summer. Beside these, a number of seedling plants were distributed at random in different gardens; and what gave me no little satisfaction, I observed in *June*, near the large plant of all, a cluster of young seedlings coming up from a head which had been supposed rotten, and dug into the border at its winter cleaning. The result in autumn was very similar to that of the former year, but with a still more decided advantage to the plant in screened gravel. One of these expanded its first flower the 19th of *August*, and its last the 27th of *September*; all the seeds ripening perfectly: being the dark purple variety ζ, and planted singly in the middle of an open grass plat, it attracted far more attention than the venerable *chestnuts*, *magnolias*, *Cembra pines*, *cedars*, and *cypresses*, relicts of Peter Collison's labour, which surrounded it. The largest plant of all, against the south east side of the house, in rich earth, had not opened a single flower on the 14th of *October*, when I left the place; but though plants at *Holland House*, as well as in Messrs. Lee and Kennedy's garden at *Hammersmith*, had then been already blasted by the frosty nights, I understand it remained

Seedling plants.

Flourished best in screened gravel.

ed uninjured, and continued blowing till the middle of *November*, in great beauty.

Mill-hill.

High grounds warmer in winter than valleys,

but cooler in summer.

It is necessary to observe, that the Village of *Mill Hill*, where I lately resided, is situate upon a high ridge, at the head of two vallies, in which some of the sources of the little brook, called the *Brent*, arise; and the garden, in which these *dahlias* were cultivated, is well screened from the weather by high trees. Being rather above the level at which the exhalations of the adjacent country pass off, the early autumnal and late spring frosts never reach it; at least they have been so mild during the six years I lived there, as never to have cut off *cucumber plants, potatoes, french beans*, and *tropæolums*, till long after others of the same species had been killed in the vallies. In hoar frosts, the top of *Harrow Hill, Bushy Heath, Elstree*, and *Totteridge*, are commonly seen green, or illuminated by the sun, when the rest of the neighbourhood is white as snow, or obscured in a sea of fog. The medium temperature of this delightful spot, and I believe of most other grounds equally elevated, during the months of *December, January* and *February* is considerably milder than in any valley, perhaps never less than from 1 to 6 degrees: in extremely severe frosts, the difference is still more apparent, so that when the cold has been down to 12 and 9 degrees of Fahrenheit's thermometer in *London*, it has only been 20 and 16 there; and this is likewise proved by the more tender exotic plants still remaining in the garden, some of them 60 and 70 years old. The common broad leaved *myrtle* against a wall there quickly grows to 6 feet in height without any covering, and the *cupressus sempervirens*, as well as *arbutus unedo*, are rarely scorched, and never killed. The summer temperature of *Mill Hill*, on the contrary, is as much cooler than that of the vallies, as its winter temperature is milder; and it suffers greatly in dry seasons from the want of those dews, which refresh the latter; both circumstances unfavourable to the success of such perennial plants as the *dahlias*: nevertheless they have apparently succeeded better here than in any other place. No intelligent gardener, after reading the foregoing detail, can be at a loss how to treat these plants, nor is there the smallest doubt, that by check-

Caution. Earwigs.

ing the luxuriance of the herbage, their flowers may be brought to perfection, even in situations the most exposed to autumnal frosts. I have only one caution to give, which is, that in whatever soil they are planted, but especially if it is poor and gravelly, they must be duly watered in dry weather, till the flower bud can juſt be discovered in the heart of the leaves; after which they will require none whatever. Insects do not appear to attack them much, except the *ear-wig*, and for this I know no remedy but personal labour in catching and destroying them: that their numbers and consequent havoc however may be wonderfully diminished in the course of a few years, by moderate exertions, I have twice experienced.

2d and 3d species. Their introduction. Specific names. These more tender. Management.

The seeds of the second and third species, *dahlia sphondyliifolia*, and *bidentifolia*, were also sent along with those of the first, from *Madrid*, in *May*, 1804, by the Right Hon. Lady Holland: but one plant of the *dahlia sphondyliifolia* had been previously introduced from *Paris*, by E. J. A. Woodford, Esq., and flowered in his garden at *Vauxhall* in the autumn of 1803. The same reasons for which I have changed the specific names of the first species oblige me to offer new ones for these two. Cavanilles' names are actually false: for no variety of the second has yet been seen with rosecoloured florets, or of the third with scarlet; so that on this account Dr. Sims has very justly hesitated to quote him in the *Botanical Magazine*; and a yellow variety having since been introduced, it is become doubly objectionable. Both these species are more tender and flower later than the first, so that they require with us every help which art can give to forward them in spring. The best method I can suggest, and which succeeded at *Mill Hill*, is to keep them always in pots, except a plant is wanted to be much branched for making cuttings. After the first day of *April*, accelerate their growth in a very airy frame, exposed as much as possible to the light, but with very little or no bottom heat from dung; and about the middle of *June* plunge the pots close to a south east or south wall, nailing up the branches as they shoot. All the secondary branches should be pinched off while tender, with the finger and thumb, and even their principal leaves partly cut off, if the plants are disposed to be very luxuriant. Just water enough to keep them growing must be given daily, but no more. I surely need not add, that in such heavy rains as sometimes fall here after the summer solstice, they must have no water at all; but rather, if possible, be protected from their violence by a glass light.

X.

Some Questions in Ornithology. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Queries respecting the imber goose.

AS I know not where an inquirer will be more likely to meet with information on any points of natural history than from the many scientific readers of your valuable Journal, I should take your inserting the following as a favour, and would be much obliged to any of your readers, who would be so kind as to elucidate the point in question.

I am, Sir,
Your obedient Servant,
S. N.

In an account of the Feroe islands, written by a Danish clergyman, a translation of which is given in Dr. Aikin's elegant work the Athenæum, I find the following passage. "The imber, or ember goose, colymbus immer, is one of the most beautiful birds in Feroe. It is as large as a common goose, and lives constantly on the dry land; and although it has been often seen with grown up young, no person has ever yet found its nest. As it has a large hole under each wing, many have imagined, that it there hatches its eggs. The change in regard to the beautiful ring around the neck observed in some of these birds is in all probability a consequence of their different ages."

Peculiarity mentioned.

This large hole under each wing I do not find mentioned by any ornithologist I have at hand; I should be glad to know therefore whether it really exist; and, if it exist, for what purpose it is intended. I can scarcely suppose it to be for the purpose of hatching its eggs, as in this case, I think the bird must have been caught occasionally with an egg in it. Lewin, who studied more from nature than from books, says in his beautiful work, the Birds of Great Britain, that we have no information to be depended upon of the manner of breeding of the divers, which he makes, I conceive with propriety, a distinct genus. He conjectures, however, that they make a floating nest on retired waters like the grebes.

Inconsistencies in other parts of the description.

I would farther remark, that the first and last sentence in the paragraph above quoted seem more applicable to the northern diver, colymbus glacialis, than to the immer: but the assertion in the second sentence, that "it lives constantly on dry land," appears altogether inapplicable to any of the diver genus. Lewin, speaking of this genus says, "if we consider the situation of their legs at the extreme part of the body, and their having scarcely any thighs, we must be convinced, that they were not intended for walking: we cannot suppose therefore, that these birds are inhabitants of the land, but that they breed and live on the water only." In fact, if nature webbed their feet for them to live on dry land, she has done something in vain.

XI.

On the Gold Mines in the Department of the Isère, by HERICART DE THURY, *Mine Engineer**.

French gold mines known to the old writers.

THE existence of gold mines in France has long been questioned, though old writers have given us the most positive accounts of them; and the auriferous sands have been considered as our only sources of this metal. The want of accuracy in terms, uncertainty of place, vague information, and mysterious air of the mountaineers, who are naturally mistrustful, have long prevented credit from being given to the repeated stories of discoveries of gold mines: but when the intendants, by order of the regent duke of Orleans, caused a general search to be made into all the metallic and mineral matters throughout the kingdom; when collections of these substances were formed, and they were carefully described; and when accurate analyses had been made by enlightened chemists; the wealth concealed in the French territories could no longer be questioned. Dauphiny was then cited as one of our richest provinces; its mines attracted the attention of government; information considered till that time as vague and uncertain was collected with care; skilful men were employed to examine these indications, and it was soon found, that Dauphiny really possessed several gold mines, some of which appear to have been known and worked in very remote times.

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* Abridged from the Journal des Mines, vol. XX, p. 101.

Dauphiny one of the richest provinces in mines.

Two kinds of them.

The mines are of two different kinds: some affording native gold; others containing this metal mixed or so intimately combined with different metallic substances, that its presence is to be detected only by the assay.

Native gold mines.

The native gold mines of what was formerly the province of Dauphiny are, 1, that of la Gardette; 2, that of Dormillouse or la Freissinière; 3, those of Orel; and 4, the auriferous sands of the Rhone. The mine of Dormillouse is in the present department of the High Alps, and that of Orel in the department of the Drôme.

Mountain of la Gardette.

The mountain of la Gardette rises above the village of the same name, in the commune of Villard-Eymont, near four miles south of the town of Oisans, and about six from Allemont. Its mine was included in the circle of mines granted to Stanislaus count of Provence, brother of Lewis XVI, by a decree of the council of state in 1770.

Mines granted to the count of Provence.

Geology of the mountain.

This mountain, which is 1290 met. [1410 yards] above the sea, and 550 m. [600 yards] above the town of Oisans, has at its foot a perpendicular cliff above 200 m. [218 yds.] high. Its base is a reddish granite, composed of red feldspar, green steatitic quartz; and gray mica. Above this is a laminar quartz rock of a blackish gray, the strata of which run S S E and N N W, and have an inclination of 33°.

Gold vein.

This micaceous rock, in which the vein of gold is found; is covered by a secondary limestone, which forms the whole of the upper part of the mountain. This is of a deep blue gray, and contains belemnites and ammonites. The inclination and direction of its strata vary greatly, but in general they incline to the north at a greater or less angle, which appears to be determined by the slope of the primitive rock, on which this limestone rests. At the southern extremity of the mountain, below Villard-Eymont, the micaceous rock is covered by amygdaloid hornstone, which from the decomposition of its calcareous nodules assumes a pseudovolcanic appearance.

The vein of la Gardette is quartz in mass, crystallized wherever the siliceous matter has not been sufficient to fill the whole of the vein. It is enchased in gneiss. Its direction is W N W; its dip to the south, 80°; its thickness varies from 60 to 80 or 90 centim. [1 ft. 11 inch. to 2 ft. 11 inch.] and upwards. Its length has been ascertained for more than 450 met. [492 yrds.] from the foot to the summit of the mountain.

Attempts to work it.

The first attempts to work it were made in the beginning of the last century by some of the mountaineers, who gave it up for want of money or of skill. In 1733 some researches were made by order of the king, but they were badly conducted, and without success. In 1765 some of the inhabitants of Gardette attacked it anew, to obtain rock crystal. Their labours were confined to a shaft of 11 met. [12 yds.] deep, in which they found some indications of gold, in crystals of sulphuret of lead deposited on needles of quartz. In 1770, after the discovery of the silver mine of Chalanches, one Lawrence Garden tried the vein, and after several days labour he found in the gangue many specimens of gold distinctly marked. These he carried to the foundry of Allemont. Mr. Binelli, its superintendant, assayed them, ascertained the presence of the gold, and paid a visit to the spot; but he could not be persuaded, that the specimens he had assayed were taken from this vein. In 1779 however, Mr. Binelli being succeeded by Mr. Schreiber, Garden carried some specimens to this gentleman, who visited the place, and found, that the vein really contained gold. On the report of Mr. Schreiber, the count of Provence ordered the mine to be explored, and the works were begun in

June 1781, and they were continued with much diligence till 1788.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Cryolite.

AT the meeting of this Society on the 11th of February, Professor Jameson read a short account of the oryctognostic characters and geognostic relations of the mineral named cryolite, from West Greenland.

Rare species of whale.

Mr. P. Neill read a description of a rare species of whale, lately stranded near Alloa, in the Frith of Forth. It measured 43 feet in length; had a small dorsal fin; longitudinal sulci in the thorax; short whalebones *(fanons)* in the upper jaw; the under jaw somewhat wider, and a very little longer than the upper; both jaws accuminated, (at least, considering the bulk of the animal, they might be so described), the under one ending in a sharp long ridge. From these characters he considered it as evident, that it was the *baleinoptera acuto-rostrata* of la Cepede, and that that author had fallen into an errour in saying, that this species never exceeds from 26 to 29 feet long.

Great sea snake.

At the same meeting, the Secretary laid before the Society several interesting communications. 1. Copies of the affidavits made before the Justices of the Peace at Kirkwall in Orkney, by several persons who saw and examined the great sea snake *(halsydrus Pontoppidani)*, cast ashore in Stronsa in October last; with remarks illustrative of the meaning of some passages in these affidavits.—2. An account of the discovery of a living animal resembling a toad, inclosed in a bed of clay, in a cavity suited to its size, at the depth of 57 fathoms, in the coal formation at Govan; communicated by Mr. Dixon, of Govan Hill.—3. An instance of remarkable intrepidity displayed by an old male and female otter, in defending their young, although the otter

Toad in a bed of clay.

Intrepidity of an otter.

otter is generally accounted a very timid animal; communicated by Mr. Laskey, of Crediton.

Collection of British shells.

At this meeting also, Mr. Laskey (who is at present with his regiment at Scotland, and who is well known in the scientific world as an eminent conchologist), presented to the Society a very valuable and well arranged collection of British shells, and likewise a curious mineral from New Holland.

Royal Society.

Royal Society.

IN the Bakerian Lecture read in December last, before the Royal Society, Mr. Davy has given an account of various experiments on ammonia, sulphur, phosphorus, charcoal, the diamond, plumbago, and the fluoric, boracic, and muriatic acids. The results seem to prove,

Ammonia contains oxigen.

1. That ammonia contains oxigen, which he had ventured to infer from facts detailed in his Bakerian Lecture for 1807; and that in the action of ammonia and potassium, it is the ammonia that is decomposed, and that there is not the slightest evidence in favour of the existence of hidrogen in the metal of potash.

No hidrogen in potash.

Sulphur and phosphorus.

2. That sulphur and phosphorus contain hidrogen and oxigen, and that they are probably combinations of these matters with bases, which have never yet been obtained pure; and that they are analogous in their constitution to the oily bodies, except that these last have for their bases the carbonaceous element,

Carbon.

3. That charcoal in its purest form contains hidrogen; and the diamond probably a small quantity of oxigen; and that the purest form known of the carbonaceous element seems to be in plumbago, where it is alloyed with iron.

Fluoric acid.

4. That the fluoric acid may be decomposed and recomposed, and that its basis is analogous to the sulphureous basis.

Boracic acid.

5. That the boracic acid is likewise susceptible of decomposition and recomposition; and that it furnishes a peculiar basis, which is more analogous to charcoal than to any other species of matter.

Muriatic acid.

6. That muriatic acid gas in its common form contains at least one third of its weight of water; and that, when

the substance is obtained free from water, it is more conducting as to electricity, does not redden vegetable blues, and in different combinations detonates violently with potassium. Whether the muriatic basis be separated in these explosions, Mr. Davy has not yet been able to ascertain.

Nitrogen decomposed.

In a communication read before the Royal Society, February the 3d, Mr. Davy has detailed various experiments on the distillation of substances formed by the action of potassium on ammonia, which he conceives cannot be explained on any other supposition, except that nitrogen is a compound of oxigen and hidrogen, or that ammonia and water consist of the same kind of *ponderable* elementary matter. Whichever conclusion may be finally adopted, the decomposition of nitrogen in the process is sufficiently evident.

Correction of an errour.

By an errour of the press in the extract from Mr. Davy's letter in our last number, 1000 is put for 100. The context sufficiently shows this mistake; but for greater security I notice in this place, that 100 plates are quite sufficient for decomposing potash.

TO CORRESPONDENTS.

Dr. ROBERT HARRINGTON, of Carlisle, must allow me to decline any engagement as to publishing his intended communication, till after it may have been submitted to my perusal. I must also beg leave to say, that neither the threat nor the insinuations contained in the letter, which conveys the proposal I have rejected, are, in my apprehension, entitled to notice.

I am much obliged by the communication of Mr. Kemp, which I am prevented from making use of, because his researches have been anticipated; as may be seen in the 2d edition of my Chemical Dictionary, and in Rees's Cyclopedia, Art, Copal.

METEOROLOGICAL JOURNAL

For FEBRUARY, 1809,

Kept by **ROBERT BANCKS**, Mathematical Instrument

in the STRAND, LONDON.

JAN. Day of	THERMOMETER.				BAROMETER, 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.		Night.	Da
27	48	48	46	46	29·51	Cloudy	Rain
28	48	48	50	46	29·59	Ditto	Fair
29	46	47	48	45	29·26	Fine	Rain
30*	45	46	48	43	29·32	Rain*	Ditto
31	42	42	44	40	29·76	Cloudy	Fair
FEB.							
1	46	50	52	48	29·77	Ditto	Ditto
2	48	49	51	47	29·51	Rain	Rain
3	50	45	52	42	29·21	Fair †	Ditto
4	44	43	46	42	29·45	Ditto	Ditto
5	43	43	46	42	29·38	Ditto	Ditto
6	44	46	48	36	29·42	Rain ‡	Ditto
7	38	35	40	31	29·89	Cloudy	Ditto
8	32	33	34	33	29·90	Rain & hail	Cloud
9	42	48	50	44	29·39	Ditto	Rain
10	47	47	50	44	29·28	Cloudy	Ditto

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1809.

ARTICLE I.

Plan for an improved Theatre: by SIR GEORGE CAYLEY, *Bart. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR, *Brompton Malton, Yorkshire, March* 1, 1809.

Plan for a theatre.

IN consequence of this second theatrical disaster within so short a period, I take the liberty of enclosing, for the use of your valuable Journal (if you think proper), a plan, and letter of explanation respecting it, which I submitted to the proprietors of Covent Garden Theatre some time ago, but which I am just informed by Mr. Kemble has not been able to be adopted in any degree by Mr. Smirke, their architect. If you think the paper worth printing, or any extracts from it (which might be more proper) it is much at your service; if not, you will have the goodness to return it. As a new theatre will soon have to be built, and as the public is so much interested in having it as convenient as possible, publishing a few hints of this kind, however imperfect, may call forth others of more value; and thus at length some good may arise.

Imperfect hints may be improved upon.

voice by reflected sound.

faces of the building, to those
where the direct voice of the ac
be heard distinctly. There sho
this effect in some part of the l
lar sounds uttered in successio
separately by the finest ear, wl

No echo would be heard within certain limits.

tween each does not exceed $\frac{1}{10}$
reflected portion of a sound tl
less than $\frac{1}{10}$ of a second after tl
separate impression, or echo, b
of the former impression. Hei
1142 feet per second, unless
whole distance travelled over by
of the direct pulse be greater tl
be perceived. In the plan of t
difference of this sort only amo
even in this instance, about one
reflected pulse would be exerte
of the sensation of the direct p
prolonging a nearly similar imp

I must apologize for writing
net: I am much hurried, bein
ney, and I conceive, that, unl
your next number, they will
theatre requires planning. I
thank you for much pleasure a
me by your Journal.

To JOHN KEMBLE, Esq.

SIR, *Brompton, Sept.* 25, 1808.

Theatres should be contrived for convenience of seeing & hearing.

SINCE the lamentable accident, that has so lately happened to the Covent Garden Theatre, the frequent occurrence of that event to my thoughts has led me to speculate upon the various improvements, that might be made in the construction of theatres. I have taken the liberty of enclosing you the following plan and hints, which I conceive to be worthy your attention, in as much as they state undoubted principles, which local convenience may more or less permit to be put in practice, but without an attention to which no theatre can be pronounced well constructed. The science of acoustics is perfectly well understood, and the enclosed rough sketch of the internal plan and elevation of a theatre is modified to the principles of that science, in conjunction with giving the greatest possible convenience of sight to the largest number of people the space can contain.

Advantages of the elliptical form.

It is the property of an elliptical room, to collect all the sound uttered in one of its foci into the opposite focus by reflection; hence, as the ellipsis is a very beautiful curve, and as it is only the parts of a theatre distant from the stage, that require the aid of reflected sound, I have adopted this figure, as the ground plan, Pl. VIII, fig. 1, will show. Here any voice uttered upon the stage at A would be concentrated at the point B, excepting what is absorbed by entering the side boxes.

Semicircular stage.

Advantages & disadvantages of seats concentrical with it in the boxes

I have drawn the stage semicircular, and on one side arranged the seats concentrical with it. This, I conceive, would be a material benefit to the observers, but it would have this objection, *viz.* that the seats, if so placed, must rise in steps and have arms to each; hence the necessary allowance of room for the accommodation of the largest persons would be more than necessary for smaller ones; and on no occasion, however pressing, could the advantage be taken of sitting closer.

Scenes should be segments of circles.

I have also drawn the scenery in a portion of a circle, which would be a most material advantage, both to the hearing and sight, *if conveniently practicable:* and provided double the height of a scene can be had within the build-

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Sir George Cayley's Plan for a Theatre.

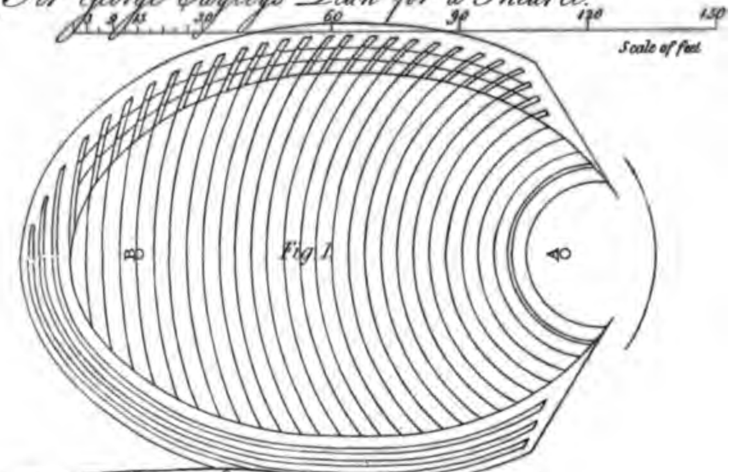

Fig. 1.

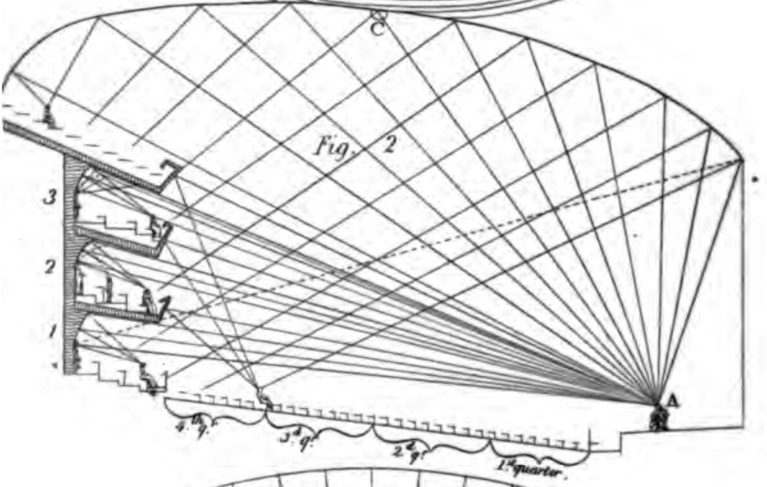

Fig. 2

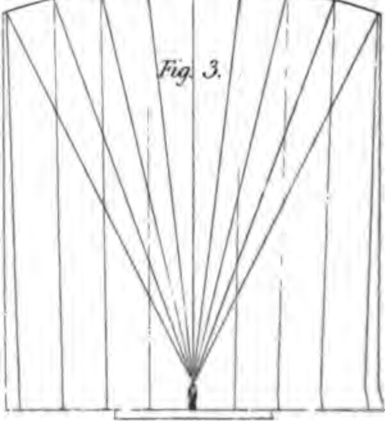

Fig. 3.

çades. one of which is the principal subject of this memoir.

The line of coast, that bounds our basaltic area on its north side, extends about twenty-five Irish miles, in which course the precipices are nearly continuous, and more than one half of them absolutely perpendicular for a great part of their stupendous height. The operation by which they were cut off so abruptly, and left with a formidable aspect towering over our coast, is the one we inquire into.

They once projected farther.

That these bold precipices once projected farther in many places is easily demonstrated; at *Beanyn Danes*, and at the *Chimney*, the columnar construction was obviously once carried much farther out.

At the *Milestone*, *Portcooan*, and *Portnaban*, the fragments of dikes extend far beyond the face of the precipice.

Not formed by the sinking of a part, or any violent convulsion,

These same facts, together with the projecting base, show, that these sudden abruptions were not formed by the subsiding and sinking of one part, leaving the remainder in its place: still less by any violent revolution, or convulsion; as the stratification has not sustained the slightest shock either above or below the façade.

or by the beating of the waves.

The formation of our abrupt coast has been ascribed to the action of the sea beating violently against it, washing away the lower parts, and leaving a perpendicular façade standing; as we often see on the banks of rapid and encroaching rivers.

For the cliffs are too high,

A cool examination of our precipices will soon prove, that our facades could not have been so formed, for we always find them in the highest part of the cliff, and receding from the water, which could be instrumental in bringing down the materials from above only by washing, and so wearing away the bases of the steepest parts; but the elevations of these bases are utterly irreconcilable to this supposition; for instance, the base of *Pleskin* façade is two hundred feet above the present level of the sea, that of *Fairhead* three hundred; now had the sea ever risen to either height, it would have submerged a great part of *Ireland*, and none of the neighbouring country (whatever its level may be) bears the least resemblance to alluvial ground, nor shows any mark of having been once covered by the sea.

The

and occur equally inland.

The next argument is still more conclusive; the boundary of our basaltic area on its north side is for twenty-five miles also the confine of sea and land; so far it is natural to ascribe its features and characteristic marks to the action of the powerful element that beats against it. But when that precipitous boundary ceases to be the confine of sea and land, turns southward towards the interior, and becomes the line of demarcation between the basaltic and schistose country on the west, it still preserves its former character; that is, of a range or ridge of very high land, steep to the exterior, and sometimes cut down vertically into façades, like its northern part that lines the shore.

Instances.

Thus *Magilligan Rock*, (four miles inland) is not inferior in magnificence to any of our façades on the coast, its perpendicular section is one hundred and seventy feet, and this continuous for a mile; the façades at *Bienbraddock* are nine miles farther inland, and those of *Monyneeny* thirteen; while the base of the lowest of these perpendicular precipices is elevated 1400 feet above the sea.

The same style prevails on the east side of our basaltic area, after its boundary ceases to be the confine of sea and land; for the limestone façades at *Garron Point*, (considerably above the level of the sea) exactly resemble those of *Dunluce* and *Kenbaan* at the water edge; and *Cave Hill* (several miles from the sea, and nearly one from the shallow estuary of *Belfast*,) exhibits basaltic façades at the height of one thousand feet, precisely similar, and little inferior to those of *Magilligan*.

These resemble those on the coast.

The exact resemblance between our inland façades (on the east and west sides of our area), to those on the shore, proves them to be all effects from the same cause, and that our accumulated strata have in all these similar instances been cut down vertically by the same agent, and that this agent was not the sea.

Similar appearances in the interior of the basaltic country.

Nor has this powerful agent confined its operations to our coast, or to the periphery of our basaltic area; we can trace it over its whole surface; we find throughout its interior, similar, though very diminutive abruptions, executed precisely in the same manner, that is, strata cut across by a long vertical façade, their planes on the upper side perfectly undisturbed, while on the lower side all the materials of which that part of the stratum was once composed are completely carried off.—(See 6th fact.)

We are now unavoidably led into a discussion of a question, which has at all times occupied the attention of naturalists.

Whence arise the Inequalities, with which the Surface of the Earth is so exceedingly diversified?

Whence arise the inequalities on the surface of the Earth?

I shall not attempt to encounter this question generally, nor to extend my inquiries beyond the limits I have prescribed to myself; but I shall try whether the curious facts, so profusely exhibited over our basaltic area, throw any light upon the formation of our own inequalities, or lead us a step toward the discovery of the operations, by which such stupendous effects have been produced.

Some say from original formation,

Some, to escape the difficulties in which this question is involved, ascribe our inequalities to original formation; as if the world had come from the hand of the Creator with the variegated surface, which now contributes so much to its beauty. But the frequent interruptions and resumptions of the strata in our area, with the perfect resemblance of the corresponding parts, however great the interval by which they are separated, can scarcely leave a doubt, that these strata were at first continuous; of course the figure of our surface at that time must have depended on the original positions and inclinations of these strata, which, as appears by the 3d fact, are now unconnected with the superficial line, the figure of which is governed by their abruptions and removals alone.

others from causes within or on the surface of the Earth.

Naturalists have differed much in opinion as to the direction, in which the causes acted that produced the inequalities on the surface of our globe; some referring us to the bowels of the Earth as to the scene of action; while others assert, that the operations were performed upon the surface itself.

But the slightest inspection of our façades will at once prove, that the first hypothesis cannot be correct; for obliquity of direction must have been the result of a disturbing cause acting from below; whereas parallelism and a steady rectilineal course distinguish the basaltic arrangements of which I have been treating.

Sometimes strata have subsided from failure of their supports.

We have, it is true, occasional depressions of our strata, where they obviously have subsided, and no doubt from a failure of support below; but in no instance that I have met with, in our area, are these attended by the slightest concussion; the permanent and subsided parts, with us, still preserve their parallelism, and the continuity of their material; whence it is probable this event took place previous to the induration of the strata, and of course antecedent to the period, to which I limit myself.

Buffon's hypothesis.

Buffon ascribes our superficial inequalities to the agitation of the waters while they covered our Earth, and argues from the resemblance these inequalities bear to the waves of the sea; a resemblance I cannot trace in any country, which I have observed; nor could any sudden and perpendicular abruptions ever have been produced by any agitation of the waters.

Playfair's.

Professor Playfair considers rivers as having formed not only the beds, or channels in which they flow, but also the whole of the vallies through which they run, and in general all the inequalities of our surface; but an attentive observer, tracing the course of any of our most rapid rivers, would soon perceive, that the quantity of its depredations have been comparatively insignificant, and that they can be determined with precision; the river has no doubt in several places extended itself considerably on both sides; but in the intermediate space, between the remotest boundaries it ever reached, it levels, instead of raising inequalities.

Decay and decomposition.

The same result I apprehend would follow from the operations of another agent, which theorists are in the habit of calling in to their aid, when they cannot find some certain material, which from their theory we have reason to expect; they then tell us it has been carried off, and lost in the suite of degradations and decompositions.

But they would wear down the prominences.

But delay and decomposition, instead of creating inequalities, would produce a contrary effect, and deface those actually existing; they would gradually abate the height of our perpendicular façades, and increase the green steep at their bases by the accumulation of the crumbling and mouldering

originally, is not only striking, but where also
tant circumstances afford demonstration, th
operation has once taken place there.

Thus, by making ourselves acquainted wi
shall be better qualified to investigate causes;
effects shall appear to be beyond the powers
ral agents as we are already acquainted with, we
fied in admitting the performance of operati
we have seen nothing similar; and also in adm
mer existence of powers of far superior ener
have ever known in action.

Enumeration of some remarkable Inequalities of our basaltic Area, produced since the C its Strata.

Remarkable inequalities produced since the consolidation of the strata.

That we may better understand the facts I
ing to state, I shall assume (in the style of th
cians *puta factum*) previous to demonstrat
planes of our uniform, rectilineal strata, howe
ed we may now find them, were once continuou

Upon this supposition, the valley of the *Ma*
the stratified summits of *Seafin* and *Slievegal*
cavation 1700 feet deep, and three miles wide,
whole materials have been completely carried

On the eastern side of our area, immediately to the southward of *Kello* and *Connor*, a similar operation has been performed, attended by still more extraordinary circumstances.

Remarkable inequalities produced since the consolidation of the strata.

We here find a district near four miles in diameter, called the *Sandy Braes*; over this whole space the basaltic stratification has been carried off, and the operation has reached deep into a very singular substratum; a reddish breccia, by some mineralogists called a porphyry, the mass friable, but the component angular particles of extreme hardness.

The hills, of which this little district is full, are every one perfect segments of spheres; while the loftier basaltic hills that surround it preserve their characteristic form, to wit, a gradual acclivity on one side, with a steep abruption on the other.

As we sail along our northern shore we discover another great chasm or interruption of our strata, which also cuts deep into the substrata: for on the west side of *Ballycastle* pier, the bold basaltic precipices suddenly disappear, and at a lower level disclose the substratum, which appears to be an alternation of sand-stone and coal, sometimes with bituminous schistus.

A mile or two to the eastward the abrupt precipice is resumed, and a basaltic stratum again occupies its summit on to *Fairhead*, with the same angle of inclination in which it was disposed along our whole coast, that is a slight ascent to the north.

Traces of similar operations and abruptions are to be found over our whole area, but the preceding are sufficient to make us acquainted with the style of these interruptions of our strata; of course it is time to proceed to the suspended demonstration, that our strata, so interrupted, were once continuous, notwithstanding the magnitude of the interval by which the corresponding parts are now separated.

Proofs that our now interrupted Strata were once continuous.

We must now turn back to the façades of *Bengore*, where the strata themselves, and all the circumstances attending them, are so happily displayed, as to throw great light

Proofs that the interrupted strata were

once contiguous as far as the great depress
Pleskin.

Here indeed the interruption becomes
less than a mile; but when we find at *Port*
of three strata with the same inclination, in
of the same thickness each, and with the
racteristic marks, that distinguished the th
at the depression; above all, when we find
rest upon continuous (at least with very
tions) for the same extent; I think we can
a doubt, that this interval between the con
though so much greater than any of the p
them, but an interruption, and that these
continuous from the depression to *Portmoc*

The same style of induction would est
dam continuity of all the strata in the face
montory, for here the strata are so distinc
we know each of them, when we find it ag
terruption.

In the rest of our precipices and façades,
the strata deprives us of this advantage; y
interruptions, the eye, by tracing the rec
the strata, and so connecting the sepa
establish their former continuity: while i

geological construction of our area,—the strata of which it s formed—the arragement—and their inclinations.

Proofs that the interrupted strata were once continuous.

An extensive limestone stratum, white as chalk, and about two hundred feet thick, seems to form the base of the whole district I limit myself to: upon this accumulations of rectilineal and parallel basaltic strata are heaped up to most unequal heights.

This great calcareous stratum seems not to be an accurate plane, but rather to resemble a basin, as every where at its periphery it dips to the interior; yet the plane of its section has a slight ascent to the southward. A recollection of these circumstances will enable us to account for every appearance this stratum exhibits, as it happens to be disclosed to us; and by the converse, an attention to these appearances will enable us accurately to determine the position of the stratum.

This stratum, from *Ballycastle* to *Solomon's Porch*, (about twenty-five miles,) keeps very nearly the level of the sea, often indeed sinking below the surface, but never raising its lower edge above it; but when, at *Solomon's Porch*, the boundary of our basaltic area begins to deflect to the south-west, and then to the south, the ascent of the stratum to the southward begins to operate, and we perceive the dotted line of its quarries gradually to rise along the face of the mountain from the shore to *Mounyneeny* and *Scafin*, where it has attained the height of 1500 feet. It is true, the actual stratum has not been opened at these two great elevations, but the white rubble immediately below the basaltic façade proves incontestibly, that it is close at hand.

An accumulation of basaltic strata had in this southern course, as well as on the northern shore, covered the limestone up to the summits of the hills or mountains.

I have already stated how the ridge of mountain is suddenly interrupted by the valley of the *Mayola*, from 1600 to 1700 feet deep; but if we look to the southward, in the rectilineal course of the strata (the positions of which we have been able to ascertain with so much accuracy), we shall find near the summit of the mountain *Slievegallon* a similar white limestone stratum crowned with basalt, cutting it in the very direction the former ought to have reached

it,

the coast; but now interrupted by the valle
like our former more diminutive interruptic
them resumed at the next elevation, in the
course, the strata preserving the same orde
characteristic marks. Or whether these s
on the summit of *Slievegallon*, be the com
new arrangement, abandoned by nature as
which is highly improbable, for neither lim
is to be found on the mountain, except in t
mock.

We might, by a minute attention to the
arrangements of the strata contiguous to th
tions I have enumerated, prove in like man
saltic masses crowning the summits of t
hills and mountains are merely the rem
once extensive and continuous, but interru
off, as in the preceding case, by the same p

The more diminutive inequalities scattere
surface of our area, and always produced l
of the strata, would still more easily admit
of the same reasoning, from the contiguity
ed parts; but the detail would be tedious;
to pursue the subject farther must come to
selves.

Materials completely carried a

The removed parts of the limestone stratum on the west side of our area have shared the same fate: for where the chain of mountains extending from *Magilligan Rock* to *Bienbraddock* is interrupted by the vallies at *Stradreagh*, *Drummommer*, and *Ballyness*, it is obvious that the limestone stratum was once continuous to the high points where it shows itself on *Keady*, and the mountains on each side; its thickness too, wherever we can try it, is very great; yet this stratum, which in its entire state must have spread like a roof far above the present surface of these vallies (which are now sunk deep into the schistose substratum) has not left a particle of its *debris* behind, nor is a single lump of white limestone to be found, until we come to the quarries, that is, to the edge of the solid, untouched stratum.

Conclusions.

General inferences.

The conclusions that unavoidably follow, from the consideration of these facts, are,

That the hills and mountains, in the district I have been describing, were not raised up or formed as they now stand, but that they are the undisturbed remains of strata that were left behind, when stupendous operations carried away the parts that were once contiguous to them.

That the inequalities of this surface were all produced by causes acting from above, and carrying off whatever they touched, without in the least disturbing what was left behind.

Additional Evidences. Basaltic Hummocks[*].

Additional evidence in the basaltic hummocks.

The arguments on which I have founded my opinions have hitherto been all taken from the hollows in our surface, and the interruptions in our strata, both which the concomitant circumstances have led me to consider as so many excavations; but the lofty elevations, and the abrupt prominencies rising suddingly from our surface, when minutely examined, lead us irresistibly to the very same conclusion.

When you and I examined together the line of our northern façades, we studiously sought for the points where

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[*] Navigators use the word hummock to express circular and elevated mounts, appearing at a distance; I adopt the term from them.

nature

Additional evidence in the basaltic hummocks.

nature had made any change in her materials or their arrangement, hoping that at the junctions of these little systems we should find some facts, that would throw light on the subject; but we generally failed; want of perpendicularity, or other local circumstances, impeding us at the most interesting points.

On the present occasion she has adopted an opposite line of conduct, and in many of the steps she has taken, obtrudes upon us demonstration of what she has done.

Whoever has attended to the exertions of man, when employed in altering our present surface, either by levelling heights, or by making excavations, must have observed that it is the practice of the workmen to leave small, cylindrical portions standing, for the purposes of determining the height of the old surface, and thereby ascertaining the quantity of materials removed.

To these may be compared the stratified basaltic hummocks so profusely scattered over our area, and which seem to show how high our quondam surface once reached.

The hummock of *Dunmull*, three miles south-east from *Portrush*, is very beautiful; it stands on the top of a high ridge, and is a conspicuous object from all parts of the country; it is exactly circular, its flat surface contains an acre, it is completely surrounded by a perpendicular façade about twenty-five feet high, and formed by two strata, a columnar, and an irregular prismatic resting upon it.

From this elevated station, where I had the pleasure of accompanying you, I showed you at six or seven miles distance to the westward, among the *Derry* mountains, the still loftier hummocks of *Altabrian* and *Sconce*, hemispherical in form, composed of but one stratum each, while their swelling-out bases displayed accumulations of many more: and also near the hummock of *Fermayle*, resembling *Dunmull*, but much larger, and also surrounded by a façade composed of two strata.

I showed you also at twenty miles distance to the south-east the gigantic *Slemish*, one of our basaltic hummocks, magnified (as it were) into a lofty and insulated mountain, completely stratified from its base to its flat summit.

I showed you likewise from the bottom of its ridge the

next

neat but diminutive hummock called the *Rock of Clogher*, above *Bushmills*. As our time was precious, you took my word for its stratification being precisely similar to that of *Dunmull*.

Additional evidence in the basaltic hummocks.

There are many other basaltic hummocks scattered over the surface of our area, all of them either stratified or portions of strata; two of the most remarkable are the hill of *Knock Loughran*, near *Maghera*, and a tall hummock (the name of which I forget) a mile eastward from *Lismoire*.

We meet still more frequently an imperfect style of hummock, a semicircular façade on one side, while on the other it slopes away gradually with the dip of the strata, as if the operation had been interrupted before it was carried quite round; the most remarkable of these are *Ballystrone*, in *Derry*, and *Croaghmore*, in *Antrim*, both visible from *Dunmull*.

Regular stratifications on the summits of hills and mountains have long been a stumbling block to theorists. The historian of the French Academy, for the year 1716, obviously ascribing the superficial inequalities of the Earth (like many others) to causes acting from below, and perceiving how incompatible such assemblages of strata were to his theory, thinks it safer to doubt their existence, as they could not have been formed, he says, "unless the masses of the "mountains were elevated in the direction of an axis per-"pendicular to the horizon: *ce que n'a pu être que très* "*rare*."

But as these stratified mounts are in our area by no means uncommon, they lay us under the necessity of suggesting another alternative similar to those we have already stated.

Were these isolated hummocks originally formed as they now stand, (solitary and separate from each other) one by one? or are they the last remaining portions of a vast consolidated mass, of which the intermediate and connecting strata have been carried off by causes, with which we are unacquainted?

To be able satisfactorily to resolve this alternative, it becomes necessary, to take a careful view of the contiguous countries, and to try whether their construction, and the ar-

grand façade of *Magilligan Rock*, three mil
plays an accumulation of exactly the sam
consolidated into an enormous mass.

The hummock of *Dunmull* is formed of
cular strata, a columnar, and an irregular p
showed you, a mile to the northward, at t
quarries of *Islamore* and *Craigahuller*, at t
hill, that the whole ridge, on the summit of
is placed, was a consolidated mass, forme
strata of the same description: and that the
the whole country below, and adjacent, w
same with that of the hummock of *Cloghe*
you at the curious opening of the strata
Bridge, and in the façades at the *Giant's Ca*

After these proofs, that so many (and I m
the rest) of our detached hummocks are in
tion and materials similar to, and connected
consolidated masses of which our countr
think it will scarcely be asserted, that th
were originally formed solitary and separa
stand; but rather that they were once pa
whole, and left behind at their present form
moval of the contiguous portions of their s
powerful agent, of whose operations and m

Additional evidence in the basaltic hummocks.

And in the façade of *Magilligan*; the highest of all, a few desultory patches of an upper stratum (no doubt once perfect and continuous) are to be traced along its summit.

Our mountains themselves seem to show clearly, that they were once higher; the top of *Magilligan* mountain is an extensive plain, over which a wandering stratum is interrupted and resumed at intervals for a great way.

At the highest part of *Donald's Hill*, over the valley of *Glenuller*, we find a hummock; also at the termination of the ridge, at its highest part over the valley of *Mayola*, similar hummocks. I have the honour to be, Sir,

Your obedient, humble Servant,

Clonfecle, Jan. 2, 1808. W. RICHARDSON.

III.

Remarks on the Habits of the Imber and Northern Divers, in answer to a Correspondent. In a Letter from THOMAS STEWART TRAILL, *M. D.*

To Mr. NICHOLSON.

SIR,

Author of the account of the Feroe Isles mistaken.

IN article X of your last number, a correspondent wishes for information concerning the habits of the *Colymbus Immer*. The observation of the Reverend Author of an Account of the Feroe Isles must be incorrect, when he asserts, that "the imber lives constantly on the *dry land*."

Habits of the imber and northern divers.

Both the colymbus immer, and c. glacialis are frequently met with among the Orkney and Shetland isles, where I have had many opportunities of observing them swimming about with great velocity, and experienced the difficulty of shooting them, from the celerity with which they dive on the flash, and the very great distance they swim under water.

Similar story of its incubation in the Orkneys and Feroe islands.

The inhabitants of these islands have never seen either of the two species of diver above noticed on the dry land; and, to explain the incubation of a bird which they believe never quits the water, have had recourse to the story of its hatching its egg under its wing, as well as the natives of the Feroe isles. As the egg has never been found in such a situation by any one, it is not easy to discover the origin of such a story among the inhabitants of the Feroe isles, if they believed that the diver lived constantly on the *dry land*. It seems obviously to have originated among them from a similar cause that produced it among the inhabitants of the Orkney and Shetland islands, a people till lately connected with those of the Feroe isles by frequent intercourse and common language.

This story fabulous.

The truth is, I believe, that the two species of colymbi here noticed live almost constantly on the water; when they leave it, it is only for the purpose of incubation; and they then seem, from the best accounts, always to choose some sequestered spot very near to their favourite element. Indeed the position of the legs of all the divers, and the structure of their feet, show that they are totally unfit to live constantly, or even principally on the dry land. I have never seen either the c. immer or c. glacialis attempt to fly; nor did I ever hear any one say, that they had observed them do so. When closely pursued they swim with great ease and rapidity, and dive for a much longer time than any other bird I recollect to have observed.

They never fly, but swim rapidly, & can stay very long under water.

I am, Dear Sir, yours very truly,

Liverpool, 1809. THOs. STEWART TRAILL.

IV.

Inquiry concerning the Use of Galvanism in Deafness. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Application of galvanism in cases of deafness.

IT appears, that galvanism is successfully applied in some cases of deafness; and particularly in such as cannot be relieved by other means. I am so fully persuaded of your desire to render the Philosophical Journal subservient to objects of utility, that I presume to request you to invite your correspondents, to transmit a particular account of the mode of applying galvanism in cases of deafness.

I am, Sir,

Your most obedient servant,

Feb. 1809. T. V.

V.

An Addition Table, with a Multiplication Table on a new Plan. By Mr. CHARLES HAYTER.

SIR, *Dec.* 27, 1808.

Addition table.

Improved multiplication table.

MY son desired me to make him an *addition table*; the thought was new to me; but trying, I produced the enclosed, and since have done the multiplication table in the form herein presented. Several *teachers* have complimented the plan; because, when the Tables are learnt, the manner of setting down the sum is also perfectly taught, which is not the case with any other multiplication table.

The multiplication table of 144 squares is well adapted to stand at the beginning of duodecimals; and I have not the conceit to suppose the form I have produced will be any where preferable, but in the hands of very young beginners. Your opinion, Sir, of the composition will oblige

Your obedient servant,

CHARLES HAYTER
Portrait Painter.

42, *Margaret Street,*
Cavendish Square.

Addition seldom well learned.

ANY means for facilitating the early acquirement of knowledge must be of value to society. It is certain, that though all educated persons can multiply figures very well, yet there are few, except those in counting-house, who can add with rapidity and certainty. This author's Table, if well fixed in the memory, appears to afford a remedy for the last evil. But I would observe, that the square multiplication table has the advantage of giving products readily, when the factor is the largest of two numbers; so that a boy thus taught can as readily say 8 times 4 is 32, as 4 times 8 is 32, which it is well known cannot at first be done by one who has learned only the half-table with braces. It would be an improvement in both tables of Mr. Hayter, if they were thus completed.

Complete tables best.

W. N

AN ADDITION TABLE.

One and one	1 1	2 1	3 1	4 1	5 1	6 1	7 1	8 1	9 1
make	2	3	4	5	6	7	8	9	10
Two and two	2 2	3 2	4 2	5 2	6 2	7 2	8 2	9 2	
make	4	5	6	7	8	9	10	11	
Three and three	3 3	4 3	5 3	6 3	7 3	8 3	9 3		
make	6	7	8	9	10	11	12		
Four and four	4 4	5 4	6 4	7 4	8 4	9 4			
make	8	9	10	11	12	13			
Five and five	5 5	6 5	7 5	8 5	9 5				
make	10	11	12	13	14				

A MULTIPLICATION TABLE.

one Twice	1 2	2 2	3 2	4 2	5 2	6 2	7 2	8 2	9 2	10 2	11 2	12 2
make	2	4	6	8	10	12	14	16	18	20	22	24
three Three times			3 3	4 3	5 3	6 3	7 3	8 3	9 3	10 3	11 3	12 3
make			9	12	15	18	21	24	27	30	33	36
				4 4	5 4	6 4	7 4	8 4	9 4	10 4	11 4	12 4
				16	20	24	28	32	36	40	44	48
					5 5	6 5	7 5	8 5	9 5	10 5	11 5	12 5
					25	30	35	40	45	50	55	60
						6 6	7 6	8 6	9 6	10 6	11 6	12 6
						36	42	48	54	60	66	72

Six and six	6 6	7 6	8 6	9 6
make	12	13	14	15
Seven and seven	7 7	8 7	9 7	
make	14	15	16	
Eight and eight	8 8	9 8		
make	16	17		
Nine and nine	9 9			
make	18			

NUMERATION TABLE.

Units	1	One
Tens	2 1	Twenty one.
Hundreds	3 2 1	Three Hundred and twenty one.
Thousands	4, 3 2 1	Four thousand, 3 hundred and 21.
Tens of Thousands	5 4, 3 2 1	Fifty 4 Thousand, 321.
Hundreds of Thousands	6 5 4, 3 2 1	Six Hundred and 54 Thousand 321.
Millions	7, 6 5 4, 3 2 1	Seven Millions, 6 Hundred and 54 Thousand, 321.
Tens of Millions	8 7, 6 5 4, 3 2 1	Eighty 7 Millions, 6 Hundred and 54 Thousand, 321.
Hundreds of Millions	9 8 7, 6 5 4, 3 2 1	Nine Hundred and 87 Millions, 6 Hundred and 54 Thousand, 321.

7 7	8 7	9 7	10 7	11 7	12 7
49	56	63	70	77	84
	8 8	9 8	10 8	11 8	12 8
	64	72	80	88	96
		9 9	10 9	11 9	12 9
		81	90	99	108
			10 10	11 10	12 10
			100	110	120
				11 11	12 11
				121	132
					12 12
					144

The intention of composing these Tables in their present form is to convey to the Learner a perfect idea of the forms of the Sums in the Rule, which no former Tables have done. Observe, each Article in the Tables is a perfect Sum in the Rule to which it alludes.

VI.

On the Cultivation of the common Flax, Linum Usitatissimum *of Linné, as an ornamental Plant in the Flower Garden. By* Mr. JOHN DUNBAR, *Gardener to* THOMAS FAIRFAX, *Esq.*[*]

Flax cultivated as an ornamental flower

THE Horticultural Society will perhaps honour with their attention a short paper, the object of which is to bring into cultivation the common *flax*, as an ornament of the flower garden, not merely as such, but with a view to the profit it will afford, at least to the servant, if not to the master; and the interest of the former can seldom be promoted in an honest way, without some benefit accruing to the latter. This plant, when so cultivated, like wax and honey, forms part of the natural riches of a country, and if it could supplant the cumbersome yellow *lupine* in our flower borders, the annual revenue arising from it would amount to several thousand pounds.

would add to the wealth of the nation.

Five persons supplied with linen from one garden in this way.

If gardening were in its infant state among us, a complete treatise on the culture of this plant might be necessary; but as this is not the case, only what is especially material will be noticed, with some directions how to prepare the plant after it is gathered. They are the result of several years experience, by which a family consisting of five persons has been supplied with all the linen they required.

A loamy soil best.

The soil of every flower garden is always rich enough to produce good *flax*; but if it is loamy rather than sandy, the quantity will be nearly double: even in the fields, which can never be cultivated with the nicety of a gentleman's garden, I have observed the greatest crops in a loamy soil, and that they yielded an article superior in quality as well as quantity: for as the durability of the fibre depends in some measure upon its size, there can be no doubt that tall and vigorous plants are preferable to small ones.

Mode of disposing it for ornament.

There are various ways of disposing this plant so as to be exceedingly ornamental, but none more so than scattering

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[*] Trans. of the Horticultural Soc. p. 71.

ing it in random parcels, or little clumps of from 10 to 20 plants, towards the back of the flower borders, and in the front of the shrubbery: for, without the summer proves uncommonly dry, it will attain to the height of three or four feet. If a temporary edging, or summer screen is wanting for any particular bed, it may be also employed for this purpose.

Cultivation. The seeds of good flax are short, plump, thick, very oily, and of a bright brown colour. The best season for sowing them, in most gardens, is February, or the beginning of March, when the general crop of hardy annuals are put in; but if the ground be sandy, and naturally dry, they should be sown in *October* or *November*. No more attention than what is necessary for the other flowers in the garden, which is keeping down all weeds while in the seed leaf with a hoe, will be requisite for this. Gathering. As soon as the seed begins to ripen, and the plants turn yellow, pull the whole up by the roots, and lay it in bundles exposed to the full sun, if the weather is fine, to dry completely. Then pull the heads off, and shake out the seeds. Maceration. Immediately after, it must be laid to macerate in a ditch, or pond of water, and kept under by a long piece of timber floating upon it. From five to ten days is the time necessary for its immersion, and after the fifth, it must be examined daily, taking especial care that it does not lie too long. As soon as ever you find the fibres are sufficiently macerated to separate from one another kindly, spread it out to dry upon a new mown meadow. Preparation. When dry it must be again collected into bundles, and either sent to the flax dresser, or prepared for spinning at home by the gardener's wife. In many districts, this operation is well understood, and if carefully performed, homespun linen from such *flax* will last twice the time of most of the Irish linen that is now to be purchased in our shops.

Cautions. I believe it is a great errour to pull the *flax* so green as is commonly practised, and a still greater to soak it in water, before it is previously dried: for the fibres require twice the time to macerate sufficiently for separation in the dressing; a process by which they are considerably weakened.

VII.

VII.

Analysis of a Mineral Water near Dudley, in Worcestershire. By Mr. W. WELDON.

Mineral water near Dudley. ABOUT two miles to the south of the town of Dudley, upon the estate of the Right Hon. Lord Dudley and Ward, is a spring of mineral water, that has been famous, in the surrounding country, from time immemorial, in various scrofulous and cutaneous diseases. It is said to have been used with great success in what are called obstructions of the abdominal viscera, and in various cutaneous diseases; but in all cases of scrofula it has been considered as an almost infallible remedy.

The high character that it has obtained, and the great success that is said to attend the use of it, in the diseases before mentioned, has induced an opinion in the neighbourhood, that it deserves to be more extensively known.

In a disease that under one form or other is so very general throughout Britain, it is very probable, that this mineral water may prove a valuable and useful remedy, and deserving of a more general attention than hitherto it has met with.

Like most other mineral waters it would be most advisable to administer it on the spot; but where circumstances render this inconvenient, it may be sent for and used at home.

I am not informed that any body, living on the spot, undertakes to answer orders for it; but if any physician or surgeon should be inclined to recommend it, or if any patient, who may wish to try it, should be at a loss how to obtain it, on a personal application, or by letter, post paid, I shall be ready to give the address of a person, who I doubt not will send it at a moderate expense.

Geology of the country. I am not personally acquainted with the geology of the surrounding country. I am told it is very interesting. It appears to be of what Werner names the independent coal formation. Coals occur in great abundance; one stratum particularly is said to be ten yards in thickness; with iron stone, basaltes, clay, &c.

The spring.

The saline spring flows into a well, near a ridge of high land, on the sides of which, at some short distance, coals and iron stone are seen cropping out. The well is about 36 feet in depth, and 7½ feet in diameter. The sides have lately been fenced, to keep out foreign water, which was supposed to run into it, with a dam of bricks set in clay, and lined with elm boards.

The bottom is a ferruginous argillaceous sandstone, through which is perforated a hole, whence the water issues, and rises to within about four feet from the surface.

The water.

The sides of the well near the top are covered with a yellowish ochrey substance. When the water is fresh taken up, it is perfectly transparent and colourless. It is little refractive of light, nor can it be said to sparkle; but after standing for a short time, numerous small bubbles of air are seen adhering to the bottom and sides of the glass it is taken into. After a time the water becomes rather turbid, and at length a pale ochreous precipitate falls down, leaving the supernatant water transparent.

Its smell.

In large quantity the water smells of sulphuretted hidrogen, but if half a pint or less be examined apart, the odour is hardly perceptible.

Temperature.

When the temperature of the atmosphere indicated 40° of Fahrenheit, some water just taken from the well raised the thermometer to 47½°. At another time, when the temperature of the surrounding air was 75°, water taken from near the surface of the well lowered the thermometer to 56°, and a portion taken from the bottom to 52°.

Taste.

The taste of the water very much resembles the taste of sea water.

Specific gravity.

The specific gravity of this water is found to differ at different depths from the surface. It differs also very considerably at different times. The following are the dates and specific gravities.

July 18, 1806, taken from the bottom of the well sp. gr. to dist. water as 1018 to 1000.

Jan. 23, 1808, from the bottom of the well sp. gr. as 1028 to 1000.

Oct. 27, 1808, sp. gr. as 1013·5 to 1000.

Dec. 2, 1808, two portions of the mineral water were sent to me, one of which was taken from about the middle of the well, the other from the bottom.

Sp. gr. of the water from the bottom 1013·5.

Sp. gr. of the water from the middle 1012.

Foreign water was excluded.

In consequence of some freshwater springs being found to discharge themselves into the well, it has lately been emptied and walled round as above stated; and it is probable that hereafter the strength of the water will be more regular.

Action of the water upon lime.

It seems, that the workmen experienced a great deal of difficulty, in securing the dam. At first, they built the dam with common lime mortar, the action of the water destroyed its cementing power. Then a fresh wall was built, with a gray lime, which the neighbourhood affords, and which sets very firm under water; but this also, as might be expected, failed, from the action of the water on the lime. At length the dam was formed by setting bricks in a strong clay, that is free from lime or nearly so, and it seems to answer the purpose intended.

Examination of the water from the bottom of the well.

The mineral water that was subjected to the following experiments was taken from the bottom of the well, by sinking empty bottles provided with valves, as soon as the bottles were drawn up, they were carefully corked, and tied down with bladder.

Its appearance.

1. A glass bottle full of this water was perfectly transparent, and without sediment while uncorked. After standing for some time in the laboratory, where the thermometer stood at 40°, the cork was drawn, a small portion of gas compressed in the neck of the bottle escaped, but no bubbles of air were seen to escape or even to form for some minutes after. At length however the water became turbid, and gradually deposited an ochrey sediment.

Tested with tincture of red cabbage:

2. *a.* Pure distilled water was tinctured with a certain proportion of tincture of red cabbage, and placed as a standard of the colour.

b. An equal quantity of the tincture of cabbage was added to a corresponding portion of the mineral water, which was quite fresh. At first it became slightly reddened, but ut the tint gradually went off, and in three hours the colour was distinctly blue.

c. An equal quantity of the mineral water, which had been oiled and filtered, produced no change of colour of the ncture of cabbage at first, but when examined ten minutes afterward it was slightly blued.

litmus:

3. a. Pure distilled water coloured with a certain portion tincture of litmus as a standard.

b. The fresh mineral water in the same proportion prouced the slightest tint of red. It soon vanished.

c. The mineral water which had been boiled and filtered oduced no change.

infusion of rhubarb:

4. a. Pure distilled water was tinged with infusion of rhuarb in given proportion as a standard.

b. The fresh mineral water tinged with an equal propor-on was very slightly deepened.

c. The boiled water tinged as above could not be distinuished from pure distilled water.

infusion of brasil:

5. Corresponding experiments were made with infusion brasil. Both the fresh and the boiled mineral water prouced the slightest tint of blue.

sirup of violets:

6. Sirup of violets was changed to a green by the fresh ater. The boiled water produced no change on it.

infusion of galls:

7. a. A few drops of infusion of galls added to the fresh ineral water produced a violet colour, the water gradually came opaque, and in twenty-four hours had thrown down rather copious precipitate of a violet red colour. The liuid above was transparent and colourless.

b. The infusion of galls produced no change in the minel water after boiling.

limewater:

8. a. Lime water in very small quantity produced a pale hrey precipitate in the fresh mineral water.

b. On adding to the fresh mineral water an equal part of ne water, a white precipitate formed, which dissolved ain in nitrous acid.

sulphuric acid, strong and diluted:

9. Strong sulphuric acid rendered the fresh water turbid, d disengaged a number of air bubbles. An abundance sulphate of lime formed.

10. A few drops of diluted sulphuric acid was added to ne of the mineral water, which had been boiled and filtered,

tered, and was previously warmed. No flavour of sulphuretted hidrogen was perceptible.

muriatic acid: 11. The mineral water previously boiled was treated, as in 10, with muriatic acid; no change was produced in its transparency, nor was any flavour perceptible except of the muriatic acid.

leaf silver: 12. Silver leaf left in contact with a portion of the fresh water, in a closed phial, for several days, was not perceptibly tarnished.

nitrate of mercury: 13. Nitrate of quicksilver produced a copious white precipitate.

oxide of bismuth: 14. *a*. Oxide of bismuth mixed with the fresh water in the proportion of 4 grains to 4 oz. was very slightly darkened.

b. With the boiled water no change of colour was produced.

acetate of lead: 15. *a*. Acetate of lead threw down from the fresh water a dense white precipitate. When the precipitate had wholly subsided, the surface of it was of a pale ochre or fawn colour, apparently produced by the oxide of iron only.

b. With the mineral water, after being exposed to the atmosphere for a few hours, and decanted, the precipitate was a pure white.

nitrate of lead: 16. Nitrate of lead threw down a dense precipitate of a pure white colour.

nitric acid: 17. Nitric acid produced no change in the fresh water.

nitrate of silver: 18. Nitrate of silver produced a dense white precipitate, which was kept for several days in a dark room, without undergoing the least change of its colour.

acetite of silver: 19. *a*. A few drops of acetite of silver threw down a precipitate of a rather dull white colour. It was kept in a dark room for 24 hours, when it appeared somewhat darkened.

b. The boiled water treated in the same manner did not darken the acetite of silver.

muriate of mercury: 20. *a*. Muriate of quicksilver, added to the fresh water, produced no immediate change. After a few minutes the water became slightly opaque, and a slight precipitate at length appeared. It scarcely exceeded the quantity of oxide of iron which the water contained, but the colour was of a redder tint than the ochrey powder which the water let fall spontaneously.

b. The same salt added to some of the mineral water which had been exposed to the atmosphere for some days, and decanted, produced no change in it.

21. Oxalate of ammonia instantly produced a copious white precipitate.

oxalate of ammonia:

22. Oxalic acid produced the same.

oxalic acid:

23. A solution of pure ammonia threw down a precipitate.

pure ammonia:

24. Equal parts of the mineral water and of fresh made lime water were mixed together, and the precipitate that was thrown down was separated and washed. It was dissolved in muriatic acid, precipitated by subcarbonate of soda, washed, and dried in a low heat. Distilled vinegar dissolved a part of it. A solution of pure potash dissolved the greater part of the remainder, leaving a brown powder.

lime water:

25. Acetate of barytes produced no precipitate in the filtered water.

acetate of barytes:

26. Muriate of barytes produced no precipitate in 24 hours.

muriate of barytes:

When the saline contents of the water were concentrated by evaporation to one fourth, a slight precipitate formed.

27. Muriate of strontian produced no precipitate in the water, either when fresh, or when concentrated as in 26.

muriate of strontian:

28. Phosphate of soda produced a copious precipitate.

phosphate of soda:

29. Carbonate of ammonia produced a white cloud.

carbonate of ammonia.

30. A crystal of carbonate of potash did the same.

carbonate of potash:

31. *a*. In the fresh water succinate of ammonia produced a reddish brown precipitate.

succinate of ammonia:

b. In the boiled water a few drops of succinate of ammonia produced no change.

32. *a*. Prussiate of potash and iron produced a deep blue precipitate in the fresh water.

and prussiate of potash.

b. In the boiled water this triple salt produced no change.

33. A portion of the fresh water was boiled in a glass retort, the curved arm of which was placed under an inverted jar filled with quicksilver. The gas which was thus collected was tried with nitric acid, the acid absorbed a part of it.

Gas expelled by boiling and tested with nitric acid, superacetate of lead,

34. A portion of gas produced as in 33, was exposed to the action of a solution of superacetate of lead, by which a part of it was absorbed.

& lime water.

35. Another portion of this gas, and also the residua of the experiments 33 and 34 were severally exposed to the action of lime water. A corresponding proportion from each of the gasses was absorbed.

Precipitate from boiling treated with muriatic acid.

36. The precipitate obtained by boiling a quantity of the fresh mineral water to about one half was washed, dried, and treated with dilute muriatic acid. A part of it only was dissolved.

37. A solution of pure ammonia threw down a white precipitate from the muriatic solution in 36.

38. Oxalate of ammonia occasioned a precipitate in the same solution.

The residuum examined.

39. The powder which resisted the action of the muriatic acid in 36 was boiled in subcarbonate of soda, and then in a solution of pure potash. A part of it was dissolved by each.

40. The powder left undissolved by the last operation was boiled in muriatic acid. What remained was melted in a large proportion of pure potash. Silex was precipitated on the addition of distilled water.

The water evaporated to dryness.

41. A quantity of the fresh water was evaporated to dryness. The dry mass was treated with successive portions of alcohol, the specific gravity of which was 815°. The residuum, which resisted the action of the alcohol, was dissolved in water, filtered, and slowly evaporated. The crystalline substance thus successively separated, was pure common salt.

Contents of the water.

From these experiments it appears, that this mineral water contains a small proportion of sulphuretted hidrogen, the same of free or rather supercombined carbonic acid, carbonate of iron, carbonates of lime, of magnesia, and of argil, probably carbonate of manganese also, a minute proportion of sulphate of lime, with muriates of soda, lime, magnesia, and of argil, silica, with azote, or atmospheric air.

Their proportions examined.

To obtain an approximation to their respective proportions, the following additional experiments were made.

42. Grains

Air expelled.

42. Grains by weight 22045 = 85·973 cubic inches nearly of the fresh mineral water were poured into a flask, and securely luted to a tube terminating under a graduated jar filled with quicksilver. The quantity of atmospheric air that was contained above the water in the flask and in the tube was ascertained to be 9·5 cubic inches. The thermometer was at 47° of Fahrenheit, and the barometer 29·7. The water was boiled for about half an hour. When the apparatus was cold, the whole of the air in the flask and tube was transferred into the graduated jar. When reduced to the same temperature and barometrical pressure as before the operation of boiling, it measured 16·1 cubic inches.

Tested with superacetate of lead:

43. The gas thus collected was washed in a solution of superacetate of lead, until no farther absorption took place. The quantity absorbed was 1·95 cubic inches.

lime water:

44. The gas was then exposed to the action of fresh made lime water, until it underwent no farther diminution of bulk. The quantity absorbed by the lime water was 2·1 cubic inches.

and sulphuret of lime.

45. The residual gas amounting to 12·05 was next treated with a solution of sulphuret of lime, that had been saturated with azote. The quantity of oxigen absorbed was 1·9 cubic inches.

The 9·5 cubic inches of atmospheric air contained in the apparatus consisted of 7·5 + cubic inches of azote, and 2 — cubic inches nearly of oxigen. The whole of the oxigen indicated in 45 was contained therefore in the atmospheric air. The small deficiency of oxigen appears to have been produced by that quantity having combined with the black oxide of iron, which during the experiment fell down in the form of red oxide.

2·65 cubic inches of azote therefore were given out by the quantity before stated of the mineral water.

The water evaporated.

46. 88180 grains by weight = to 343·9 cubic inches of the fresh mineral water were boiled for an hour or more and filtered. They were then evaporated to about 30 cubic inches, when the muriate of soda began to crystallize. The water was then filtered again, and the small quantity of precipitate that was then obtained, and which from a pre-

Muriate of soda crystallized.

vious examination was known not to contain any sulphate of lime, was added to the former.

Precipitates treated with muriatic acid, and caustic alkali.

47. The precipitates obtained from the mineral water by 46 were treated with strong muriatic acid, and boiled in it, which did not completely dissolve them. The undissolved powder was washed and boiled for some time in a silver crucible, with a strong solution of pure potash. The solution was diluted, the powder separated, and again treated with muriatic acid, washed, and dried in a red heat. This powder weighed gr. 2·6.

The solution precipitated, & the precipitate examined.

48. The washings were all added to the solution formed in 47, and the whole precipitated by subcarbonate of soda. The supernatant solution was neutralized and the precipitate washed. It was redissolved in muriatic acid. The muriatic solution was neutralized, diluted, and treated with prussiate of potash and iron. The precipitate thus formed was calcined to destroy the prussic acid; nitric acid was abstracted from it two or three times, and then it was dissolved in muriatic acid. The solution was high coloured, it was diluted with water, and precipitated by carbonate of potash. The precipitate was separated by filtration, washed and dried. The subcarbonate of iron thus obtained weighed gr. 5.5.

Subcarbonate of iron.

49. The liquor left after filtering off the subcarbonate of iron in the last operation, was boiled for three quarters of an hour or more; a light coloured precipitate after some time began to separate, which soon became darker coloured. This powder, which I conclude to be carbonate of manganese, when washed and dried weighed gr. 0·75.

Carbonate of manganese.

50. The muriatic solution that was filtered off from the prussiate in 48 was accurately neutralized. Oxalate of ammonia was then dropped in gradually, as long as a precipitate formed. The oxalate of lime, when washed and dried, weighed gr. 3·1 = gr. 1·6 of pure lime, or gr. 2·4 of carbonate of lime.

Carbonate of lime.

51. The solution left in fifty was then treated with pure potash. The precipitate was separated, washed, redissolved in muriatic acid, and again precipitated by subcarbonate of soda, washed, and dried in a heat of near 200°. It weighed gr. 19·75.

52. This precipitate was treated with distilled vinegar, until the vinegar no farther acted upon it. The residuum, when washed and dried, weighed gr. 3·2. Consequently gr. 16·55 of carbonate of magnesia were dissolved by the vinegar, and gr. 3·2 of subcarbonate of alumina were left.

Carbonate of magnesia, and subcarbonate of alumine.

53. The concentrated mineral water, which was separated from the earthy carbonates &c. in 46, was diluted with distilled water, and oxalate of ammonia added, until the whole of the lime fell down. The oxalate of lime that was thus obtained was well washed and dried. It weighed gr. 482·25 = to gr. 241 of pure lime = to gr. 482·25 of dry muriate of lime, taking Kirwan's estimate for the composition of the latter.

Muriate of lime.

54. Pure potash was now added to the filtered solution left in 53. The precipitate was separated, redissolved in muriatic acid, and precipitated by subcarbonate of soda. The precipitate washed and dried weighed gr. 118·3.

55. This precipitate was treated with distilled vinegar until no more dissolved. The subcarbonate of magnesia thus dissolved weighed 99·05, equal to gr. 143·45 of muriate of magnesia according to Kirwan. The alumina when heated red hot washed and dried weighed gr. 17·25 = to nearly gr. 57·3 of muriate of alumina.

Muriates of magnesia and alumine.

56. 88180 grains of the fresh water were evaporated to dryness. A quantity of muriatic gas escaped towards the end of the operation from the partial decomposition of the muriates of magnesia and alumina. Water and a small quantity of muriatic acid were now added to dissolve the soluble parts of the mass; the insoluble powder was then treated with strong muriatic acid, washed, dried, and weighed. It was now thrown into a hot solution of subcarbonate of soda, from which it was separated, and treated with diluted muriatic acid, which dissolved a minute proportion of carbonate of lime. This was precipitated and separated by a filter, but did not exceed 0·25 of a grain by computation.

Silex.

57. The insoluble powder, which could consist only of silica and alumina, was boiled in a silver crucible with a solution of pure potash, the solution was decanted off, and the

white powder that was left was washed, and exposed to a red heat. It weighed gr. 3.

Sulphate of lime.

58. In order to obtain a nearer approximation to the quantity of sulphate of lime, contained in a given portion of the mineral water, than that which was obtained in 56, where in dissolving the muriates a part of the sulphate was probably redissolved, 22045 grains of the mineral water were evaporated to about one eighth of their bulk, and filtered. A known proportion of muriate of barytes was then added. A slight cloudiness was perceived in a few minutes. Twenty-four hours after, the small quantity of precipitate, that had formed, was separated by a filter, washed, and dried. Allowing for what was lost upon the filter it amounted to 0·25 nearly.

59. 22045 of the mineral water were exposed for some days to the atmosphere and filtered: nitrate of silver was then added, until no farther precipitation ensued. The whole was poured upon a filter, and the precipitate was well washed, and dried for twenty-four hours in a stove at 212° nearly. The muriate of silver thus formed weighed 937·25 grains, which multiplied by 4 give 3749 grains for the quantity of muriate of silver formed by muriatic acid contained in 88180 of the mineral water.

60. If we suppose the muriate of silver to contain 18 per cent of acid; the quantity of muriatic acid would amount to 674·82 grains.

If grs. 482·25 of muriate of lime contain gr. 202·545
143·45 of muriate of magnesia.... 81·766
57·3 of muriate of alumine .... 17·19

Muriate of soda.

of muriatic acid; there will be gr. 787 of muriate of soda, supposing it to contain 50 per cent of base, 44 of acid, and 6 of water.

The muriate of soda found in a different way.

61. A given quantity of the mineral water was evaporated to dryness, and digested in alcohol of ·815 applied in successive proportions to dissolve the earthy muriates. The muriate of soda thus obtained was decomposed by nitrate of silver. The result of this experiment was nearly as above. The following therefore may be considered as a near approximation to the quantity of gaseous and solid contents

contained in 88180 grains or 343·9 cubic inches of the mineral water.

Contents of the water.

	Cubic Inches
Of sulphuretted hidrogen	7·8
carbonic acid	8·4
azote	10·6

	Grains.
Of muriate of lime	482·25
magnesia	143·45
alumine	57·3
soda	787·
Of sulphate of lime	1·07
carbonate of lime	3·1
of magnesia	16·55
subcarbonate of alumina	3·2
subcarbonate of red oxide of iron gr. 5·5 nearly equal to 5 grains of the subcarbonate of black oxide	5·
Of subcarbonate of manganese	·75
Of silica	3·

Hence one gallon, or 231 cubic inches of the water, contained

Proportions in a gallon.

	Cubic Inches.
Of sulphuretted hidrogen	5·24
carbonic acid	5·64
azote	7·1

	Grains.
Of muriate of lime	323·93
magnesia	96·35
alumine	38·5
soda	528·63
Of sulphate of lime	0·71
carbonate of lime	2·1
of magnesia	11·12
Of subcarbonate of alumine	2·15
of black oxide of iron	3·36
of manganese	·475
Of silica	·2

I have

At different times the water differs in gravity,

I have before observed, that this mineral water has been found to possess different specific gravities at different times, consequently to contain at different times different quantities of ingredients.

and in the proportions of its ingredients,

particularly the iron.

The specimen of the water that was sent to me in July 1806, the specific gravity of which was 1·018, was submitted to some experiments, which show, that it differs also at different times in the proportion of its ingredients. In this case the difference is very remarkable in relation to the oxide of iron.

Instance in a different portion examined.

When boiled in a retort that terminated under quicksilver, the black oxide of iron fell down in abundance, and the earthy carbonates were in very small proportion. The water that was decanted threw down a green hydrate of iron mixed with magnesia, on the addition of lime water. When the water was boiled in the open air and filtered, lime water threw down a brown precipitate.

The following is the proportion of some of the ingredients it contained. While this examination was proceeding I was obliged to suspend it for a time, to attend to more pressing avocations, and an accident prevented my completing it.

Its contents.

From a wine gallon, or 231 cubic inches, were obtained

Of muriate of soda	483·
lime	311·
magnesia & alumina	145·
iron	26·
Of carbonate of iron	9·
Of silica	·75
Of earthy carbonates about	4·5
Of carbonic acid and sulphuretted hidrogen (the latter in small proportion)	Cub. In. 23·735
Of azote	12·

The ingredients probably not in the exact state here assigned.

In both of the foregoing statements it will be seen, that I have given the ingredients as they occurred on decomposition.

But it by no means follows, that they exist in that state when in solution. It is more probable, that when in solution

tion the earths and oxides are combined with the acids, as is observed by Mons. Berthollet, in proportions depending upon a power produced by the respective proportions and the respective affinities of all the ingredients toward each other.

10, *Wigmore Street, March*, 8, 1809.

VIII.

On the Gold Mines in the Department of the Isère. By HERICART DE THURY, *Mine Engineer.*

(Concluded from p. 237.)

Description of the vein.

IT is difficult to meet with a vein so regular and well defined as that of Gardette. In fact it shows itself both on the surface and within the earth for a uniform length of 450 met.; and throughout the whole of this extent it retains exactly the same direction of SSE and NNW, without being broken off or turned aside by any failure or vein appearing at the surface.

The inclination to the depth of 78 met. [85 yds] to which the last shaft is driven, is pretty regular thoughout its whole height, being an angle of 80°. It has experienced only two slight variations; the first, near the surface, is a slight break of 15 or 20, or at most 25 cent. [6, 8, or 10 inches] with the same direction and inclination as the vein. The second has been found at 62 met. [68 yds.] deep; and is occasioned by a small vein composed of lead, copper, and sulphuret of zinc, which has turned the principal vein toward the W N W about 3 met. [near 10 feet] in that part of the vein which serves as its roof. Below this the slope has become more rapid, though its direction remains the same.

Gangue.

In all the workings the vein of la Gardette has been found to consist of compact quartz, which is crystallized wherever the silex did not fill up the whole cavity. This quartz affords very various groupes, which are very limpid when divested of the oxide of iron that covers them. The quartz constitutes the bulk of the vein, but various different metallic substances are found in it, such as sulphuret of lead, phosphated lead, arseniated lead, earthy oxide of lead, argentiferous grey copper ore, yellow pyritous copper, arseniated copper, green carbonate of copper, iron spar, sulphuret of iron, oxide of iron, acicular oxide of manganese, tellurium, &c. These substances are distinct, or combined two, three, four, or even five together. Frequently they contain gold, and sometimes this metal is apparent in them.

State of the gold at Gardette.
Native pure.

The gold is disseminated in the quartz of the vein. It is native and pure, or mixed, or alloyed with different substances. It occurs without mixture in four different forms. In octaedral crystals, adhering one to another so that it is frequently difficult to ascertain their figure. 2. In ramifications, or dendritical, the most distinct specimens of which appear to consist of little octaedra implanted one on another. 3. Capillary slender filaments between the quartz crystals. 4. Laminar, sometimes in flat, at others in twisted laminæ, the surface of which is occasionally reticulated, but more frequently like shagreen.

Mixed.

Of the apparently native gold mixed with other metals there are three varieties. 1. Ramified or capillary in crystals of sulphuret of lead. On breaking these we perceive ramifications and filaments of gold of a brilliant yellow colour. 2. Granular in sulphuret of lead. 3. Native gold in sulphuret of zinc.

Gold alloyed with other materials.

The native gold alloyed with other metals, and concealed by them, may be distinguished into eight varieties. 1. In a mixture of lead, copper, and sulphuret of zinc. 2. In the argentiferous gray copper ore with green carbonate of copper. 3. In sulphuret of iron. 4. In the sulphurets of zinc, lead, and copper, covered with arseniate of copper. 5. With tellurium in needles. 6. In oxide of iron. 7. In phosphate of lead. 8. In oxided manganese.

Gangues of the native gold.

The native gold is found likewise in five different earthy gangues. 1. In limpid hyaline quartz, or rock crystal. 2. In smoky hyaline quartz. 3. In black hyaline quartz. 4. In hyaline quartz with sulphate of barytes. 5. In hyaline quartz and carbonate of lime or calcareous spar.

Products of the gold mine.

The workings of the gold vein of la Gardette have afforded three different productions. 1. The gold which has been cast into ingots, and sent to Paris, to the count of Provence, in 1786. 2. The gold ore, which, not being sufficiently rich for the furnace, and affording instructive specimens for the mineralogist, was set aside at the suggestion of Mr. Schreiber, to be sold according to its intrinsic value to curious and scientific collectors, as is customary in the mines of Saxony and Austria. 3. The rock crystals which have been collected in the cavities of the quartz.

Method of extracting the gold.

The processes followed in the extraction of the gold are simple, and easy to conceive. The native gold requires only to be separated from its gangue and united together. For this purpose it is pounded; washed, to carry off the gangue reduced to fine powder; and triturated with ten or twelve times its weight of mercury in a mortar filled with water. This water being decanted off carries with it every thing earthy. The amalgama being separated from the earth, and perfectly brilliant, it is pressed in leather bags, to strain out the superfluous mercury from what is requisite to dissolve the gold. The solid amalgama left behind is distilled in retorts, which are brought to a strong red heat, to obtain the mercury separate; and the gold, which remains behind, is melted and cast in bars or ingots.

The gold combined or disseminated in the ores of silver, copper, lead, &c., is extracted by eliquation, cupellation, and parting. The lead that runs during the eliquation of the argentiferous and auriferous copper carries with it the silver and the gold. This mixture is cupelled to scorify the lead. The gold and silver that remain are separated from each other by parting with nitric acid.

It has been proposed, to treat the auriferous pyrites by amalgamation, the success of which, as already confirmed in Germany and Peru, proves, that the gold in them is simply disseminated, and not combined.

Not wrought since 1788.

The working of the gold mine of la Gardette has been suspended ever since the year 1788, yet it ought not to be given up. It is at present under the superintendance of the mayor of Villard-Eymont. The distance, difficulty of access, complete ruin of the miner's house, falling in of the earth, and general decay of the mine, has occasioned it to be neglected by the local authorities; and the inhabitants of the village of Gardette annually avail themselves of this desertion, as during the dead time of the year they search for gold at their own expense, and are frequently successful.

Gold sands of the Rhone.

The Rhone has been frequently quoted for its golden sands. The most ancient authors, as Pliny, Diodorus Siculus, Strabo, and Polybius, speak of the gold dust, which this river rolls along with its sands; and which the Gauls knew how to wash so as to extract the gold, of which they made rings, bracelets, and belts. Reaumur, after having given the history of rivers and rivulets with auriferous sands in the Memoirs of the Academy of Sciences for 1718, says that the gold collected in the Rhone is 20 carats fine; containing one sixth of its weight of copper and silver.

I shall now proceed to the mines of gold alloyed with different metals, by which its presence is concealed.

1. *Gold in the sulphuret of lead of Pontraut.*

Gold in sulphuret of lead:

Pontraut is a part of the chain of granitic mountains known by the name of *Petites Rousses*, above Oz and Vaujani in Oiſans. This vein is near the glaciers; it is more than two hours journey from the villages abovementioned, and in a country so cold, that it is inhabitable only four months of the year at most. The ore of Pontraut yields to the essay 58 per cent of lead: and this lead contains 122·286 gr. of silver, and 1·442 of gold, in 50000.

2. *Gold in the Sulphuret of lead of Mollard.*

another:

Mollard is a village of the commune of Allemont, situate on the right bank of the river Olle. The mine was opened by Mr. Schreiber in 1785 for the smelting works of Allemont.

This ore yields 60 per cent of lead: and 50000 gr. of the lead contain 61·143 of silver, and 1·372 of gold.

3. *Gold in the sulphuret of antimony of Auris in Oisans.*

in sulphuret of antimony:

The ore is a mixture of lead, zinc, copper, antimony, silver, and gold, united and intimately mixed. It is frequently coloured by green carbonate of copper.

It yields 50 per cent of antimony: and 10000 gr. of this

antimony

ntimony are said to contain 950 of silver, and 4·412 of old.

4. *Gold in the yellow copper pyrites of la Cochette.*

This mine is in the narrow passage of la Cochette, which rms a communication between Vaujani in Oisans and aint Sorlin in Maurienne. The height of the mine, and difficulty of access, will never allow it to be worked with lvantage. It appears, that it was attempted formerly, d a tradition of the fact is preserved; but, as is too comonly the case, the narrative has a great deal of the marellous mixed with it.

in copper pyrites:

The ore of la Cochette yields 36 parts of refined copper, d ·00236 of gold, from 100 of black copper,

5. *Gold of Theys in a pyritous copper,*

This mine is in la Combe-de-Merle, below the lake of eche-Dent, on the western slope of the mountain of Theys, elow some mines of iron spar, and in a forest of pines. It nsists of nodules of auriferous copper pyrites disseminated regularly in a vein of iron spar.

another;

The ore has never been accurately analysed. Yves Mihael du Serre, in a remonstrance to the Duke of Orleans ublished in 1651, says, "it is so pure and clean, that four arts yield three of the finest gold, and it is as plentiful as ossible."

6. *Gold of Alleyard in an argentiferous gray copper ore.*

This ore is frequently in a state of decomposition, and loured by green and blue carbonate of copper. It is und in nodules in a vein of iron spar, at Buisson near Alevard.

in argentiferous gray copper ore.

A hundred parts of the ore furnish 60 of black copper, hich yield 38 of refined copper, 4 of silver, and ·008158 of old.

There is reason to suppose, that this is the mine of which ellat speaks in his *Etat des mines du Royaume*, when he ys, that Mr. de Baral, proprietor of the iron mines of Allevard, had found a fine gold mine in that district.

7. Gold

IX

A short Account of Nectarines
duced on the same Branch.
F. R. S. &c.†

Nectarines and peaches sometimes produced on the same branch.

THOUGH it has long been
peaches are sometimes naturally
same tree, but upon one and th
the fact recorded by any author
with two instances, I presume to
anomaly to the Horticultural S
it has suggested are right or wro
by more able physiologists.

Earliest notice of this.

The first instance, of which
been handed down, will be fo
Peter Collinson Esq. to Linne,
meeting of the Linnean Society.
account of a supposed adultero
apple trees, standing near each o
quence bore both smooth and ro
tree, that produced *peaches* and

Smooth and rough apples.

Another instance

The second instance occurred
[illegible]

e Dr. Richardson, and many other horticulturists
tensive county.

rd instance is commemorated by a painting of the 3d.
Ehret, now in the possession of Messrs. Lee and
being accompanied with a dissection of the two
ch are the *alberge jaune*, sometimes called the
ich, it is very satisfactory.

rth instance was noticed more lately in the garden 4th.
Gilpin, Esq., *East Sheen*; of this likewise a paint-
ithout dissections, has been made by Mr. Hooker,
from it ascertain the variety.

h instance was discovered early in *June* last, on 5th.
f Sir John Arundel at *Huntingdon*: having never
I went there immediately, and after detaching the
efully from the wall, soon satisfied myself that no
een inserted; there was however only a single *nec-*
n the tree, which the gardener said was the *belle*
and a pretty accurate sketch of the branch is an-

th instance was in Mr. Wilmot's garden at *Isle-* 6th.
ich I also saw in *August* last, and learnt that his Tree usually
h is the *royal George*, seldom fails to produce producing
both smooth and downy coats, or in fact *peaches* both.
ines: two only of the latter then remained, and
much damaged by snails.

r to recite any others, these being more than suffi- Instance of the
stablish the truth; but my inquiries fortunately two fruits
l with the singular example now before you, of united.
s joined in one. I have to thank Dr. Batty for
identally observed it among a number of *peaches*,
a by James Wyatt, Esq., from the neighbour-
Hounslow, during our vacation; and as it was
ginning to decay, this only method of preserving
inspection was not neglected.

---

branch, the bearing wood of which is about a foot in length,
peaches, eight inches distant from each other, and between
tarine. I did not think it necessary, to have it reengraved.
ikewise given of the fruit next mentioned, one part smooth,
wny.

Most

belonging to another; but these
succeeding generation. The gr
Hybridæ and *Generatio Ambige*
demicæ, first promulgated a doctr
that varieties, species, and even
in this manner; and without the
no gardener can hope to be succ
tables, free from the faults, or en
he wishes. The pith of Linne's

Linneus's theory of the production of new kinds of plants.

getable will resemble its father, or
came, in stem and leaves; but its
the *stigma* is situate, in flowers
somewhat less restricted has be
periments, should never be forgo
sexual intercourse, every one wh
melon is well convinced, and as f
of these or other fruits is concer
in addition to modern practice,

Pollen may be preserved.

all vegetables might probably
to another; in early forcing, it
and should be kept in papers as
ing it till the *stigma* is moistened
dation. In those countries, wh
food of the inhabitants, a fami
consequence of neglecting this
trees do not flower every year, a

Brame's nursery. The common *ling*, of which our besoms are made, varies in the same way; and the *teucrium heterophyllum* takes its name from this very circumstance. I conclude therefore, that all these variations proceed from laws in vegetation, of which we are yet ignorant, but which are immediately connected with the transudation of the sap through the cuticle, and it is possible, that this may even affect the flavour of two fruits upon the same branch.

Laws in vegetation unknown to us.

X.

On the Substitution of Iron for Mahogany and other expensive kinds of Wood in Articles of Furniture, and for other Purposes. By Mr. B. Cook.

To Mr. NICHOLSON.

SIR,

As you have favoured me with inserting in your valuable Journal my former imperfect communications, I have taken the liberty, to lay before you my idea on another subject; and I leave it to you to judge, whether the idea is worth communicating to the world.

Mahogany and other wood imported at a great expense.

We import at a great expense mahogany and other costly woods for the manufacturing of the very beautiful furniture in use among the higher and middling ranks of society. The great advance of this article is felt by every one, who finds it necessary to purchase things made of mahogany. If it were possible to find a substitute for a portion of this article, were it only the half, or even a fourth part of it, it would certainly reduce the price of that part, which necessity forces us to use, in a given ratio. People of the highest order of fashion will never submit to use a substitute, unless this substitute is equal in beauty to that, the place of which it supplies. But if it were possible to find an article equal in beauty, more durable, and as cheap, we should be inclined to think it would be adopted in every case, where it was found convenient; especially if this substitute were a native of our own country.

Now

Iron abounds in this country.

We should endeavour to render ourselves independent of foreign nations.

Mahogany greatly risen in price, and much used.

Now Great Britain produces iron in abundance. I do not pretend to assert, that iron is the best substitute: if any thing else can be discovered, which will answer the end; in fact, if this communication gives birth to any idea in any other person, so that the end can be accomplished; this is all I have in view. I think, sir, that at times like the present, when all the nations of the world by turns are our enemies, it is the duty of every Englishman, to endeavour to discover, if it be possible, among ourselves, resources to satisfy our wants. Why should we, if it be possible to avoid it, be dependant on any nation? We ought to endeavour to bring into the greatest degree of usefulness every article our own country produces. By so doing, we are gaining more real wealth, than by the fluctuations and hazards of commerce. Mahogany in the last six or seven years has nearly doubled in price, and this is still increasing: and when, as is the case, every man of the middle class is imitating the luxury of the higher orders; not only imitating them in their extravagance and expensive mode of living, but in his furniture, almost every article of which in his house must be mahogany; this increases the demand to a great degree, and in proportion also the price: so that a large portion of the wealth of this country goes in purchasing in a foreign market this expensive article of luxury. If it were a native of our own soil, the evil would not be so great; for at all events the money would remain in the country, and in time would find its way again into circulation.

Iron proposed for bedsteads

Admits of much elegance and lightness.

The substitute I mean to propose is iron. In bedsteads, for instance, the posts or pillars, as well as the frame itself, might be cast hollow, beautifully wreathed up the posts with flowers, festoons, or clusters of fruit, or embossed with numberless fanciful ornaments, which the workman might touch up with his graver and chissel, to clear the foliage, &c. from the sand, and to make the flowers sharp and neat before they go to the finisher. The painter might colour them so as to have a more elegant and more handsome appearance than it is possible to give to carved wood: and besides they might be cast so light, and in such chaste symetry, as cannot be accomplished in wood. This would give employment to many of our manufacturers, as every Japanner could employ his hands in painting and polishing them; there would be ample scope for ingenuity in the cornices, and in the ornamenting and finishing them; and I think they might be sold at a considerably less price, than the carved mahogany ones are now made at.

Not expensive.

Also chests of drawers, bookcases, and bureaus, might all be made in sheet iron. The frames and mouldings might be rolled in rollers with grooves, with all kinds of patterns indented or engraved in the rollers, such as foliage, plain, fluted, or beaded stripes, or any ornamental work. The mouldings would thus be made rapidly in the extreme. The pannels might be cut out to fit the article, the fancy of the workman had made, in sheet iron. The mouldings, framing, and pannels, might then be beautifully japanned, painted, and polished, either to imitate mahogany; or with red, black, or any kind of coloured grounds; the pannels painted with landscapes, flowers, fruits, animals, or any device fancy might dictate; the mouldings might be made in all kinds of Gothic or other shapes and forms, and the pannels fitted to them, and the whole piece of furniture screwed together when completely finished. The drawers might be made with light iron framing; filled up with wire work, which would make them very light; and afterward lined with silk, cloth, paper, or any substance most convenient. This would diminish the consumption of the cheaper wood, used for the drawers, &c.

For chests of drawers and bookcases.

I do not think a piece of furniture finished in this manner would be any heavier than one made with wood. For the sheet iron for the pannels need only be of sufficient thickness, to stand to its form without bending, and the framing of proper strength to hold firmly together. Furniture made in this manner would certainly be more beautiful, and if an accident of fire were to happen, the property contained in it would be saved. The article would only want fresh japanning and painting again, if the flame had destroyed its beauty.

Such furniture not heavier than wood.

More beautiful, and a security against fire.

Large pieces of furniture, when required to be removed, might easily be taken to pieces, as all parts would be screwed together and put up again at a trifling expense, without the least injury: while at present a large, handsome, and

Convenient for removal.

valuable

valuable piece of furniture, in removal, either by the carelessness of the people employed, or its unmanageable size, is almost sure to be very much injured.

Other advantages.

Indeed there is great scope here for ingenuity and improvement; it would give employ to vast numbers of men; it would consume our own produce, that is iron; we should get an article that would endure for ages; and we should in part be independent of any other country, for the material that forms a beautiful and useful ornament in our houses.

Particularly for libraries.

Consider the advantage a scarce and valuable library, fitted up with light iron shelves and doors, would have in case of fire. The iron pannels, as well as the doors, would always fit tight, and never warp, as wood does; and if enveloped in flame, being almost if not quite air tight, it would be next to impossible, that the books or valuable manuscripts should be burnt, or so destroyed, let the fire be ever so intense, as to be lost. They might be blackened, and in part reduced to a state almost like that of the papiri at Herculaneum, were the fire to continue a great length of time without interruption; but they would not be entirely lost; and labour and patience might restore them to the world. Those valuable articles of antiquity, or indeed all valuable documents, that in their wooden cases are ever in danger of being lost to the world by fire, would be secure if preserved in iron. Modern publications, indeed, can always be restored to the sufferer, at a price; but to save those, that would be for ever lost to society, those that no money could purchase, no power on Earth could restore, is surely an object ardently to be desired.

Doors.

Doors for halls, doors of all kinds, with light iron frames, and neatly pannelled, would be neither heavier, nor dearer, I think, than those now in use; and if they should be a little heavier, custom would soon reconcile us to the use of them. In case of fire, an iron door might perhaps save the contents of a valuable room; instead of serving, as doors now do, to conduct the devouring element to the next apartment. Drawing-room doors, especially, and various other articles, if expense were no object, might be made of more beautiful and delicate workmanship, than it is possible to produce in wood. For instance, all kinds of Gothic scrolls, might be made of iron, light and elegant, and every plate or pannel, that fitted them, might be painted with any kind of device imagination and taste might devise, beautiful as the ancient painted windows of cathedrals, and they would endure for centuries.

Conclusion.

But I need not farther expatiate on this subject. If from the above hints, any one would enter upon it with that kind of spirit the thing requires, select ingenious mechanics, and study to introduce it with lightness, elegance, and taste; when it is considered, that the price in general would be less than mahogany, that it would be handsomer and more durable, that it is the production of our own nation, and that it would give employment to vast numbers of our own countrymen; I flatter myself with the idea, that in a great many instances, it would be adopted.

I am, Sir,

Birmingham, Your obedient humble servant,

Caroline Street, March 16, 1809. B. COOK.

XI.

On ascertaining Square Numbers and Biquadrates by Inspection. By W. SAINT, *Esq.*

To Mr. NICHOLSON.

SIR,

Woolwich, March 15*th*, 1809.

To ascertain by inspection whether a number be a perfect square would often save much trouble.

HAVING frequently experienced, in the solution of questions involving a quadratic equation, and more particulerly in such as relate to the diophantine algebra, a considerable degree of inconvenience and trouble from not being able to ascertain whether a number be a perfect square or not, without the tedious operation of extracting its root; I have thought, that the following rules or propositions, which I have accompanied with their demonstrations, might prove useful to many of your readers, by enabling them, on inspection, to ascertain a great variety of forms of numbers,

which

which can never be squares, and thus at least preventing them the trouble of perhaps many useless extractions. Should you, Sir, deem these propositions of sufficient importance to occupy a place in your useful miscellany, your insertion of them will oblige,

Sir, your very humble servant,

W. SAINT.

A square number cannot terminate with 2, 3, 7, or 8:

Proposition 1.—A square number cannot terminate with 2, 3, 7, or 8.

Demonstration.—The terminating figure of every product arises from the multiplication of the terminating figures of its factors. The terminating figure therefore of every square number must arise from the product of 0×0, 1×1, 2×2, 3×3, 4×4, 5×5, 6×6, 7×7, 8×8, 9×9; and these products it is evident, can only end with 0, 1, 4, 9, 6, and 5, and never therefore with 2, 3, 7, or 8. Q. E. D.

or with an odd number of noughts.

Prop. 2. A square number cannot terminate with an odd number of ciphers.

Demonstration. Since every square number ending with 0 must have its root ending with 0, such a root must be of the form 10 m, and the square number itself therefore of the form 100 m^2; where it is evident, that whatever value be given to m, the product $100 \times m^2$ must terminate with two ciphers. It is also obvious, that it cannot end with more than two, unless m^2 end with an 0; which again can only be when m is of the form 10 n, or m^2 of the form 100 n^2, and therefore $100 \times m^2$ of the form $10000 \times n^2$, which must end in at least four ciphers; and so on as far as we please. Q. E. D.

If it terminate with 4, the last figure but one must be even:

Prop. 3. If a square number terminate with 4, the last figure but one will be an even number.

Demonstration. For such a square number must have its root ending in 2, or 8; this root will therefore be of the form $10\,m \pm 2$, and its square of the form $100\,m^2 \pm 40\,m + 4$; where, whatever value be given to m, the sum or difference of the first and second terms will give an even number of tens, and an even number of tens plus 4 must have the last figure but one an even number.

Prop. 4. If a square number terminate with 5 it will terminate with 25.

If with 5 the preceding figure must be a 2.

Demonstration. For such a square number would have its root ending in 5, that is, would have its root of the form $10m + 5$, and consequently the square number itself would be of the form $100m^2 + 100m + 25$; where it is evident, that, whatever value be given to m, the sum of the two first terms will end with two ciphers, and therefore that the whole sum will terminate with 25. Q. E. D.

Prop. 5. If a square number terminate in an odd number, the last figure but one will be an even number, but if it terminate in any even number, except 4, the last figure but one will be an odd number.

If with an odd number, the last figure but one will be even: if with 6, it will be odd.

Demonstration. By prop. 1, if a square number end in an odd number, it must be in 1, 5, or 9; and by the last prop. when it ends in 5, the last figure but one will always be 2, which is an even number; and when it ends in 1, or 9, its root must end in 1 or 3, that is, must be of the form $10m + 1$, or $10m + 3$; and therefore, the square number itself of the form $100m^2 + 20m + 1$, or $100m^2 + 60m + 9$; where, whatever be the value of m, it is evident, that the sum of the two first terms in either expression will give an even number of tens; and an even number of tens plus 1 or 9 will have the last figure but one an even number. Again, if a square number end in an even number except 4, by prop. 1 it can only be in 6, and its root must end in 4 or 6; that is, it must be of the form $10m \pm 4$, and consequently the square number itself of the form $100m^2 \pm 80m + 16$; where it is evident, that, whatever be the value of m, the sum or difference of the two first terms will always give an even number of tens, and an even number of tens plus 16 must have the last figure but one an odd number. Q. E. D.

Corollary. Hence no square number can terminate with two equal figures, except two fours or two ciphers.

Vide cor. to prop. 6.

A square number cannot terminate with two similar figures, unless 4s or 0s, and not with more than three 4s.

Prop. 6. A square number cannot terminate with more than three fours.

Demonstration. For, if it could end in four fours, such a number might be expressed by $a \cdot 10^4 + 4 \cdot 10^3 + 4 \cdot 10^2 + 4$

$+ 4 . 10 + 4$; and being a square number, it would be such when divided by 4— that is to say $25a . 10^2 + 10^3 + 10^2 + 10 + 1$, or its equal $(25a + 10) . 10^2 + 10^2 + 10 + 1$, or $(25a + 11) . 10^2 + 10 + 1$, would be a square number. Now it is evident, that $(25a + 11) . 10^2$, whatever value be given to a, would end with two ciphers; and therefore, that the whole expression would terminate in two ones, which is impossible by cor. to prop. 5. Q. E. D.

No square can consist of equal digits.

Cor. Hence no square number can be contained under any number of equal digits.

Cubes.

Scholium. If this speculation be extended to cube numbers, it will be found, that such numbers may terminate with any of the nine digits, and that it is not therefore so easy to ascertain whether a number be a perfect cube from the nature of its terminating figure.

Biquadrates.

With regard to biquadrates, or fourth powers, we may observe, that, as these are square numbers also, whatever has been demonstrated above relating to square numbers, excepting prop. 1, holds equally true for biquadrates. We may farther remark, that biquadrates terminate in 0, 1, 5, or 6; and that, when they terminate in 0 or 5, their roots will terminate with 0 or 5 also: moreover, that when the root terminates in 5, the biquadrate will terminate in 625.

XII.

Some Account of Cretinism. By HENRY REEVE, *M. D. of Norwich. Communicated by* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.*[*]

Cretinism, a species of mental imbecility, endemic in some parts of Switzerland.

FELIX PLATER, in one of his observations, gives the history of a species of mental imbecility, which he saw in passing through the village of Bremis in the Valais. Cretinism, a word of uncertain derivation, is the name employed by the inhabitants of Switzerland to denote this disease, which is endemial in several districts of that country.

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[*] Philos. Trans. for 1808, p. 111.

It had probably existed long in those parts; for Plater mentions cretins as being very common both in the Valais and in Carinthia, but the peculiar marks of these wretched beings were not generally known before he described them*. Mons. de Saussure has furnished the most minute and accurate account both of the appearances of the disorder, and of the circumstances which seem to produce it; and Mr. Coxe and several travellers have noticed the symptoms of cretinism, without adducing any satisfactory explanation of the causes to which it may be ascribed. Malacarni of Turin and Professor Ackermann have given a very accurate description of several cretins that they dissected; and beside some detached essays by different authors, a very full account of this malady is to be found in an "*Essai sur le Goître et Crétinisme par M. Fodere,*" published at Paris in 1800.

Usually connected with bronchocele.

My curiosity led me some time ago to inquire more particularly into the nature and causes of cretinism, because it is usually connected with goître, or bronchocele; I was indeed led to this inquiry, partly by the hope of discovering some function for the thyroid gland, more satisfactory than what is commonly alleged; but in these expectations I have been disappointed.

Objects of inquiry.

In the summer of 1805, I had an opportunity of seeing several cretins at Martigny and Sion, and other villages in the Valais; and I was glad to compare what had been written upon that subject, with what my own observation could suggest. By inquiries on the spot, I intended to learn what connection subsisted between weakness of the intellectual faculties and the swelling of the thyroid gland: what were the moral and physical circumstances, which could influence the condition of the inhabitants, so as to make idiocy so prevalent; and what were the most efficient modes of relief. The following results I beg leave to lay before the Royal Society.

Cretinism common to mountainous countries.

Cretinism is found not only in the vallies of the Alps, both on the French and Italian side of these mountains, but in the mountainous parts of Germany and Spain; and it was

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* F. Plateri *Praxeos Medica, Cap.* III. *Basil.* 1656.

observed

Appearance of cretins.

observed in Chinese Tartary by Sir George Staunton, in a part of that country much resembling Switzerland and Savoy in its alpine appearance. The enlargement of the thyroid gland, called goitre, is the most striking feature in the unsightly aspect of a cretin; but this is not a constant attendant. His head also is deformed, his stature diminutive, his complexion sickly, his countenance vacant and destitute of meaning, his lips and eye-lids coarse and prominent, his skin wrinkled and pendulous, his muscles loose and flabby. The qualities of his mind correspond to the deranged state of the body which it inhabits; and cretinism prevails in all the intermediate degrees, from excessive stupidity to complete fatuity.

Four examined.

At a small village, not far distant from Martigny, I examined four cretins. One lad, twelve years old, could speak a few words; he was of a weak and feeble frame, silly, but had no goitre. Another boy, nine years old, was deaf and dumb, idiotic, with no goitre, the only child of his mother, who has a large goitre which affects her respiration and her voice, though in other respects she is intelligent and well formed, and the father enjoys good health; they are not natives of this place. I saw a family in which all the children were cretins; the eldest died a year ago, a miserable object; the second, a girl, twelve years old, is deaf, and dumb, and cross eyed, and has a monstrous goitre, with just intelligence enough to comprehend a few natural signs; the third is a boy six years old; small and feeble, abdomen enlarged, no goitre, very feeble in mind and body, not entirely deficient in understanding; the mother had a moderate sized goitre, but was quite free from any mental affection; the father neither goitrous nor stupid, but of a delicate constitution.

Cretinism and bronchocele not necessarily connected.

There is no necessary connection between goitre and cretinism, notwithstanding the assertions and ingenious reasoning adduced by Fodere. It is probable, the one has been assumed as the cause of the other, from the enlargement of the thyroid gland being a frequent occurrence in cretins; and as it forcibly strikes the observer from the deformity it occasions, this strong impression may have converted an accidental, though frequent occurrence, into a general and necessary cause. Cretinism is frequently observed without any affection of the thyroid gland, and that gland is often very much enlarged without any affection of the intellectual faculties. There seems to be some similarity between cretinism and rickets, as they both take place in infancy, are both characterized by feebleness of body, and sooner or later by feebleness of mind, and they both affect males and females equally; but there is no sort of connection between persons afflicted with bronchocele in England, and with rickets. For although it might be granted, that there is some delicacy of frame in females about the period of pubescence when bronchocele usually occurs, yet neither irregular formation of the bones, nor weakness of the intellectual powers, are common symptoms attending bronchocele in Britain.

Some similarity between cretinism & rickets.

Bronchocele.

To what peculiarities then, in the physical constitution of certain districts, are we to ascribe the production of this singular malady? Saussure's description of the Valais is exceedingly precise and accurate, and the causes which he has alleged appear sufficient to account for the phenomena. The vallies where cretinism is most frequent, are surrounded by very high mountains; they are sheltered from the currents of air, and exposed to the direct and still more to the reflected rays of the sun. The effluvia from the marshes are very strong, and the atmosphere humid, close, and oppressive. All the cretins that I saw were in adjoining houses, in the little village called la Batia, situate in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situate higher up the mountains, no cretins are to be seen, and the mother of one of the children told me, of her own accord, without my asking the question, that her child was quite a different being when he was up in the mountain, as she called it, for a few days.

What are the causes of cretinism?

Situation,

The production of cretinism, by the bad quality of the air and the food, the neglect of moral education, and other evils attendant upon poverty, is supported by facts so pointed, that the greater number of cases in mountainous districts where snow water abounds, may safely be ascribed

and poverty.

Not drinking snow water,

to these general causes. The notion of snow water being the cause of goître, and consequently of cretinism, seems to have been derived from Pliny (Lib. II, cap. 37,) and copied by almost every succeeding writer, because it coincides with their hypotheses of cold and crude matters, although directly contradicted by facts. In the first place, persons born in places contiguous to the glaciers, who drink no other water than what flows from the melting of ice and snow, are not subject to this disorder; and secondly, the disorder is observed in places where snow is unknown.

water from melted ice,

or calcareous water.

The theory of water impregnated with calcareous matter being the cause is equally unfounded; because the common waters of Switzerland excel those of every other country in Europe for purity and flavour. There is not a village, or a valley, but what is enlivened by limpid rivulets or streams gushing from the rocks. The water usually drunk at la Batia and Martigny is from the river Dranse, which flows from the glacier of Saint Bernard, and falls into the Rhone; it is remarkably free from earthy matter, and well tasted. At Martigny there are two or three pumps, the water of which is pure and equally fit for culinary purposes, but said to be unwholesome, without any good reason. At Bern, the water is extremely pure, yet, as Haller remarks, swellings of the throat are not uncommon in both sexes, although cretinism is rare. With regard to the alleged causes of goître, the general opinion of its being endemial in mountainous countries is of no value, because the disease is rare in Scotland, and very common in the county of Norfolk.

Bronchocele not peculiar to mountains.

Progress of cretinism.

The causes of cretinism begin to operate upon the system soon after, perhaps even before birth; the want of energy in the parent is communicated to the offspring; the children become deformed and cachectic very early in life, the growth and developement of the body is impeded, the abdomen becomes enlarged, and the glands swelled in various degrees; and the powers of the mind remain dormant, or become entirely obliterated, partly from want of proper organization, and partly from total neglect of every thing like education.

It

Little light derived from dissection.

It might be expected, that the dissection of cretins would throw some light upon the series of phenomena associated together in the origin and progress of this singular affection; but the people are so superstitious, that it is very difficult to procure bodies for anatomical examination. However, some dissections have been made, and the appearances in the cranium are very curious. From the description of a cretin's skull by Ackermann, it appears, that the cavity for the reception of the pons Varolii and medulla oblongata was completely obliterated, and that in which the cerebellum is lodged so much diminished, that it scarcely exceeded one third of its natural capacity. The return of the venous blood must have been considerably impeded by the maleconformation of the foramina. Appearances nearly similar were observed by Malacarni and by Fodere.

The skull much affected.

Description of a skull of a cretin thirty years old.

In the anatomical museum at Vienna, I saw a cretin's skull, from which Professor Prochaska was so obliging as to permit me to have two drawings taken. It is the cranium of a cretin, who died at the age of thirty, yet the fontanelle is not closed, the second set of teeth are not out of their sockets, and none of the bones are distinctly and completely formed. The head is very large, the face small; it is like the skull of an adult joined to the face of a child; every part bears marks of irregularity in the growth and formation; and irregular action must have been the concomitant of such a morbid structure, whether the appearances be considered as cause or effect.

The four angles of the os malæ are not well defined; the zygomatic and maxillary processes of this bone are wanting; the nasal processes of the superior maxillary bone are very large, and exhibit no marks of union with the os malæ; the ossa nasi are very small; the temporal bone is imperfectly formed; the zygomatic process terminates at the coronoid process of the lower jaw; the mastoid and styloid processes are wanting, and the pars petrosa remarkably small; the squamous portion not distinctly marked; the os occipitis unusually large, and numerous additional bones, ossa triquetra, along the whole course of the lambdoid suture. The other deviations of the natural structure cor-

Cretinism may be prevented.

The most decisive argument in proof o
that cretinism may be prevented by re
from the confined and dirty places wher
nursing and educating them in the hig
mountains. Within these last ten year
cretins has diminished, the condition of th
society is somewhat bettered, and more
towards that diseased constitution which i
of mental imbecility. I did not find th
tures took any pride in having any of the
or *bien heureux*, as some authors assert;
the parents were very much ashamed o
that any cretins belonged to their families
repeated attempts, only by declaring mys
sician, that I could get access into their h
any of those wretched beings in the hu
Burnt sponge is known as a remedy for t
the people where it is most prevalent; bu
ministered, because the disease is so com
not attract notice, nor affect in general th
tions of life. And as to cretinism, this s
upon as belonging to indigence and pove
place where I saw cretins, many well lo
both sexes resided, and these were, witho

Confined to the poor.

as this disease appeared in this country, in the middle of the 17th century, about the same time that Plater mentions cretinism. The origin of both names is equally obscure; and since some of the remote causes are now discovered, it is to be hoped the diseases themselves will gradually disappear, and in some happier age be known only by description.

XIII.

On the Composition of the Salts of Barytes. By Mr. ARTHUR AIKIN.

To Mr. NICHOLSON.

SIR,

Composition of barytic salts important.

THE ascertainment of the exact composition of the salts of barytes is of such essential importance to the accuracy of chemical analysis, that I shall make no apology for troubling the readers of your very respectable Journal, with the following details of a few experiments instituted for this purpose.

Muriate selected for experiment.

The salt selected by me as the basis of these experiments was the muriate of barytes, on account of its ready solubility in water, and its inalterability at a red heat. I shall begin therefore by describing the precautions, that I took to ensure the perfect purity of the substance on which I was operating.

SECT. 1. *Preparation of pure Muriate of Barytes.*

Precautions to ensure its perfect purity.

A quantity of crystallized native carbonate of barytes was digested in cold and dilute muriatic acid, till all effervescence had ceased, a portion of the carbonate remaining undissolved; the solution was then filtered and crystallized by rapid evaporation. The salt thus procured was ignited in a platina crucible, and became of a light ochrey yellow from the muriate of iron, that was decomposed. It was then dissolved in cold water and filtered, by which the oxide of iron was separated, and the liquor came through quite

portion of
it was redissolved in water, filtered
ness, and then ignited. There wa
a pure white colour, which dissolv
leaving any residue, and which I
of barytes.

SECT. 2. *Proportion of Water i*
Barytes.

Proportion of water in the crystals,

A quantity of the above muriat
water, and left to crystallize by
The salt obtained, after being
several days to the air, weighed
fully ignited for about an hour,
than that required for its fusion,
grs., which I conclude to be only
perfectly soluble in cold water.

In another experiment 100 g
that had been dried by the heat
duced to 85·5 grs. by a heat som
being then heated to a low red
grs.; it was then kept in fusion
hour, by which it lost less than 0

14·5 or 14·6 per cent.

Hence the water of crystalliza
amounts to between 14·5 and 14

SECT. 3. *Ratio between the Muri*

156·5 grs. of ignited muriate were decomposed in the same manner, and afforded 146·75 grs. of carbonate, or 93·77 per cent.

Hence (averaging the results of the two experiments) 100 grs. of ignited muriate afford 93·38 of carbonate: and 100 grs. of carbonate contain the same quantity of barytes as 106·6 of ignited muriate.

Sect. 4. *Composition of Carbonate and Muriate of Barytes.*

Composition of

93 grs. of carbonate, dried at nearly a red heat, lost 20·5 grs. of carbonic acid by solution in muriatic acid; or 22·04 per cent.

145·5 grs. of carbonate, by similar treatment, lost 31 grs. of carbonic acid, or 21·3 per cent.

the carbonate,

Hence, on an average, carbonate of barytes consists of

21·67	carbonic acid
78·33	barytes
100.	

ignited muriate,

Ignited muriate of barytes consists of

73·14	barytes
26·86	muriatic acid
100.	

crystallized muriate,

And fully crystallized muriate contains

62·47	barytes
22·93	muriatic acid
14·6	water
100.	

Hence also, 100 of carbonated barytes contain the same quantity of earth as 124·9 of the crystallized muriate.

Sect. 5. *Composition of Sulphate of Barytes.*

100 grs. of ignited muriate of barytes were added by degrees to some very pure sulphuric acid in a platina crucible. When the effervescence had subsided, the mixture was gently heated, and the excess of acid was saturated by car-

bonate of ammonia. The whole was evaporated to dryness, and then kept at a full red heat for a considerable time after all visible fumes had ceased. The sulphate of barytes thus produced weighed 110·75 grs.

and sulphate of barytes.

Then as 100 grs. of ignited muriate contain 73·14 barytes, 100 parts of sulphate of barytes contain

66·04 barytes
33·96 sulphuric acid

100.

Composition according to Klaproth.

The results of the above experiments nearly correspond with those of Klaproth, according to whom

Carbonate of barytes consists of $\begin{cases} 21{\cdot}8 \text{ carbonic acid,} \\ 78{\cdot}2 \text{ barytes.} \end{cases}$

Sulphate of barytes .... $\begin{cases} 33{\cdot}55 \text{ sulphuric acid,} \\ 66{\cdot}55 \text{ barytes.} \end{cases}$

and 100 parts of carbonated barytes yield 121·04 of crystallized muriate.

ARTHUR AIKIN.

XIV.

A Memoria Technica for double Elective Attractions. By THOMAS YOUNG, *M.D. F.R.S.*

To Mr. NICHOLSON.

SIR,

Table of double elective attractions.

I Have inserted, at the end of the Syllabus of my Lectures on the Elements of the Medical Sciences, a short table, containing the results of 1260 cases of double elective attractions. For the convenience of those who may wish to retain these results in memory, I have since employed a few leisure moments in expressing the table in the form of technical hexameters. I must beg leave to refer, for an explanation of the principles on which the arrangement is founded, as well as for a few particular doubts and exceptions, to a paper which will appear in the Philosophical Transactions; and I shall at present only observe, that each line expresses a column of the table; and that when four substances, which stand in any column, are mixed, they will arrange themselves in such a manner, that the bases will always be united to the respective acids which stand nearest to them.

I am, Sir,
Welbeck Street, Your very obedient servant,
22 *March*, 1809. THOMAS YOUNG.

CONTENTIO AQUATICA; VICTORIA; REQUIES.

Rebarisne modo posse adfore bellica rostra?
Des nautam satis apta cibo refovere alimenta;
Cor superest sanum; flabitque optatus abunde
Spiritus; has animi ira feret tibi acerrima gazas.
Ast Brontes animosus acerbo fœdere palmas
Cassus fert: ut pro rebus monet apta sodales!
Si possit, fato tubicen memor addat honores.
Postulat ossa relata, heu! flebite condere marmor.
Spes est fixa, bonum cœli gazis fruiturum.
Alma huic pax fiat orbi, lassis omnipotens des
O pater! Ut flebo jussus canere armigenûm vim!
Dire opifex belli, cesses normam abjicere omnem.
Pax fessos bona mulcet, gazas lætior auri.
Præsumam gazas nempe adfere rursus ab alto huc;
Mira dabit lucra pax, fortassis in ultima mundi.

XV.

An Abstract of a Meteorological Journal for the Years 1807 *and* 1808, *kept at Middleshaw near Kendal, in Latitude* 54° 20'. *By* JOHN GOUGH, *Esq.*

The object of the communication.

MY object in communicating the following table to the Philosophical Journal is, to turn the attention of meteorologists to two points of their favourite science, which perhaps have been too superficially examined. One of these points is the diurnal variation of temperature at different times of the year.

This is a subject of which we have a very imperfect knowledge; and the reason is obvious: for the variation in question is not easily determined by the common thermometer, at least in summer, because the sun commonly rises before the observer, and the morning observation is registered at too late an hour. To obviate this inconvenience, I made use of the thermometer invented by Mr. Six; which, with a little assistance from the observer, gives a correct account of all its variations, by noting each day the two extremes of its range.

Six's thermometer recommended.

Diurnal variation of temperature.

The diurnal variation of temperature determined by this instrument, is certainly more correct than the results of a common thermometer. As for the utility of the inquiry, I have only to observe, that meteorology is at present in its infancy; and the cultivators of the science have little to do but to collect facts for the use of their successors.

Tables of the kind here recommended, formed in different situations and latitudes, will perhaps prove necessary, when the materials of a rational theory have been collected from long observation. In the mean time we know for a certainty, that the phenomenon in question arises from the sun's annual motion between the tropics: but the influence of this luminary is disturbed in the regularity of its effects by the vicissitudes of the weather; for the variation is greatest when the atmosphere is clear, and least at the same season when the air is obscured with clouds and rain. These are manifestly causes of irregularity; but they have not sufficient power to prevent the necessary consequences of the sun's motion in the ecliptic, they only retard or accelerate his effects. For supposing the Earth to be destitute of an atmosphere, the sun would produce two maxima and two minima of variation at stated times in the course of a year, according to the doctrine of radiant heat. The minima would coincide with the coldest and hottest days, and the maxima would happen after the two equinoxes, perhaps about the times of mean temperature. These extremes appear in the table, but they are not periodical; which is also the case with the hottest and coldest days, as well as the seasons of mean temperature.

Rain collected at different elevations.

The second subject in meteorology, to which I was desirous to attend, was the comparison of the rain collected at different elevations, in the same neighbourhood. For this purpose, I made use of two gauges constructed exactly alike; one of them stood in a garden in the bottom of a valley, and the other was placed on the top of a hill almost directly north of the former, and 91 yards above it; a right line joining the two stations cannot exceed 500 yards. Negligence in one instance, and an accident in another, interrupted the series of observations in the first year, but the latter is complete; and I think the whole goes to prove the results of the two gauges to differ less in summer, than they do in winter.

As for the table, no part of it requires to be explained, except the fifth column, marked Ratio. To form this, the numbers in the third column are multiplied by 1000, and divided by the corresponding numbers in the second; so that the lower exreme of the monthly mean range is invariably denoted by 1000, and the higher by the number, which stands in the fifth column opposite to any particular month. Table explained.

Month. 1807.	HEIGHT of THERMOMETER. Least.	Great.	Mean.	Ratio.	Rain in inches. Low. G.	Upp. G.
January	31·45	40·06	35·75	1273	2·799	2·517
February	32·67	40·71	36·69	1246	4·768	1·468
March	30·29	40·93	35·61	1351	1·798	0·122
April	37·46	49·56	43·51	1323	2·839	1·804
May	44·82	58·83	51·82	1312	4·552	3·262
June	48·75	63·28	56·01	1298	2·404	2·031
July	55·11	69·14	62·12	1254	5·310	2·716
August	53·19	66·91	60·05	1257	4·219	3·214
September	40·90	54·16	47·53	1324	3·014	2·340
October	46·17	53·70	49·93	1163	6·382	5·190
November	31·18	38·45	34·81	1233	3·625	2·914
December	30·76	36·93	33·84	1200	2·246	1·564
Annual Means.	40·229	51·055	45·642	1269	Total of Rain. 38·631	29·142

Jan. 1808.	33·06	35·87	34·31	1085	4·725	2·517
February	31·16	39·76	35·46	1275	2·262	1·468
March	32·43	42·43	37·43	1308	0·160	0·122
April	34·51	46·71	40·61	1353	2·542	1·804
May	47·53	62·33	54·93	1311	3·692	3·262
June	50·98	64·06	57·97	1256	2·376	2·031
July	55·60	71·52	63·56	1286	3·388	2·716
August	54·36	66·79	60·57	1228	4·219	3·214
September	48·65	59·75	54·20	1228	3·014	2·340
October	38·30	47·93	43·11	1251	6·362	5·190
November	37·58	40·98	39·28	1090	3·625	2·914
December	33·09	38·22	35·65	1155	2·246	1·564
Annual Means.	42·10	51·36	46·73	1236	Total.	
					38·631	29·142

XVI.

*An Essay on Electrical Attractions and Repulsions; by Mr. ****[*]

Two magnetic fluids questioned.

MY experiments on magnetism, which give this property to several needles in opposite directions by a single electrical shock, led me to doubt the existence of two fluids, which, according to the theory of Mr. Coulomb, repel each other in every section of a magnet. From this doubt arose a second. I said to myself, do electrical repulsions exist without previous attraction? in other words, are there two electrical fluids, possessing with respect to each other the

Are there two electrical fluids repelling each other?

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[*] Journal de Physique, vol. LXIII, p. 378.

repulsive power of Newton[*]? Accordingly I took up the most modern treatise on electricity, and at the article "Of Repulsions and Attractions," I found six experiments, which are adduced to prove, that this attraction and repulsion are alternate. I believe, that a double affinity naturally explains this alternate movement; and I am persuaded, that the more we simplify our theories, to account for natural phenomena, the nearer we approach the truth.

1st experiment to prove alternate attractions and repulsions.

1st experiment of Mr. Libes, on attractions and repulsions, in his new Dictionary of Natural Philosophy, vol. I. p. 351.

"Rub with the hand a glass tube, so as to render it perceptibly electrical. Let fall on this tube a bit of foil, down, or any other light body; it will be attracted, and suddenly repelled by the tube. If in this last state of repulsion we follow it with the tube, it will fly off with rapidity in a given direction; but if it meet in its course with another conducting body, that is not electrified, it returns immediately to the tube, and afterward suddenly separates from it; so that if it hung freely suspended by a silk thread between the tube and the foreign body just mentioned, it would fly alternately from one to the other."

Accounted for by attraction only.

It is known, that metal has a considerable affinity for the electric matter: and it is this affinity, that attracts it toward the tube, the latent electricity of which is not merely excited by the friction, but probably some has been attracted from my body to its surface by the same cause; it being drawn toward it, to acquire as much as it can of this excited fluid. If, while it is in this state, there be any substance near, that has likewise an attraction for the same fluid, and this affinity be sufficiently strong to overcome the gravitation of the foil, it will attract it, seize in its turn the electric fluid, and convey it to the ground, which is equally greedy of it. If the intensity of the foil be still sufficient, to carry it anew toward the tube, these movements will continue to alternate, till the attraction of gravitation exceeds that of electricity. If there be no conducting surface to attract the foil, it will be attracted by the moisture of the air. The reflections I shall make on the substance of glass to explain the 4th Experiment will serve to account for the down, which is a nonconductor, acquiring in this instance an affinity for the fluid.

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[*] Optics, quest. 31: or Theory of the Earth by Mr. Delametherie, vol. III. par. 614.

I do not perceive here any repulsion: for, according to the Author himself, the metallic leaf goes to part with its electricity to the conducting substance. In the electrometer with pith balls, that diverge from each other, by which the electrical repulsion is frequently attempted to be proved, these balls separate only because the aqueous vapours of the air attract them. This is the reason of the movement of the little pendulum of Henley's electrometer, which I find to be one of the best. In fine, the divergence of all electrometers, as it appears to me, is produced by the attraction of aqueous vapours, or the action of some conducting substance, which attracts leaves and small balls saturated with electric fluid.

2d Experiment.

2d experiment.

"Suspend freely to the conductor of an electrical machine a fringe of thread twisted together so as to form a tuft; and the moment you electrify the apparatus, you will see all the threads, that were united together, separate from each other, and this to a greater distance in proportion as the electricity is powerful."

No repulsion.

Here the air, as with respect to the electrometer, attracts and separates as far as possible the threads. Every natural philosopher no doubt will agree, that the vast evaporation of water, that takes place from the surface of the globe, must impregnate the atmosphere more or less with aqueous vapour, even in what we call dry weather, and that this imparts to it a great affinity or attraction for the electric fluid. The circumambient air around the electrical machine, therefore, is that which can most readily effect its union with this fluid. That which is farther off, having the same tendency, must consequently attract toward it as far as possible every substance, saturated with it. Hence it follows, that, the more of the fluid every thread has imbibed, the more will the aqueous attraction act on it, and to the greater distance.

3d Experiment

3d experiment.

"On a plate of metal five or six inches in diameter put some shreds of gold leaf, and two inches above them let a similar plate of metal be suspended from the conductor, so as to be electrified by it. These little bits of gold leaf will be immediately attracted by the upper plate, and afterward suddenly repelled to the lower, so that these attractions and *repulsions* will continue as long as the conductor remains electrified.

"To render this experiment the more pleasing," continues our author, "we may substitute for these bits of gold leaf little painted figures, which, alternately attracted and *repelled* by the upper plate, will appear to dance between the two."

No repulsion.

This phenomenon, as the preceding experiments, is to be explained by the double play of affinities. These bits of gold leaf, or little figures, are alternately attracted toward the upper plate in consequence of their affinity for the electric fluid, and toward the lower, which has a communication with the ground, and consequently a reciprocal affinity for the fluid, till the conductor no longer contains enough of the fluid to overcome their weight. Lastly, take a fine needle with two points, that can move freely between the two plates, it will raise itself up, and stand erect, till the electricity of the conductor is exhausted.

4th Experiment.

4th experiment.

"To the conductor attach a metal rod terminating in a point. Present to this the inside of a glass, holding it in both hands; then place on a table some balls of pith of elder, cover them with the glass, and they will immediately begin to leap up against its sides. They will continue doing this for some time."

Explained on similar principles.

In this experiment there is nothing extraordinary, and it is easily explained in the same manner as the others. The substance of glass has the property of setting itself in motion, and then attracting the electric fluid, both by friction and communication with another substance saturated with this fluid: but I have observed, that to render it electrical one of its sides must be in immediate contact with a conducting substance communicating directly with the ground. Here the glass held externally between the two hands is nothing more than a Leyden phial: the pith balls are attracted by the power of affinity toward its charged sides; and the table, on which the glass rests, attracts them in its turn. Thus here again we have two attractions; whence the appearance of alternate attraction and repulsion, which continues as in the preceding experiments, as long as the interior sides of the glass are capable of furnishing a superabundance of fluid. Mr. Delamétherie has received from Glass conductor. me a glass conductor twelve or fifteen inches long, which I sent him about six months ago, to give him a slight idea of my large glass conductors. Since that time I have found, that, on holding these little tubes by the middle, and keeping them in contact with the prime conductor for a few seconds while the machine revolves, the whole substance of the glass imbibes the electric fluid like a sponge: that we have time enough to carry the point, which is a little obtuse, and had been in contact with the machine, toward the knob of a Leyden phial held in the other hand: and that touching it about twenty times is sufficient to charge it as strongly as by so many sparks from the cap of a good electrophorus. I have not yet examined the nature of the fluid it gives, a subject that deserves a place in an essay on the Leyden phial, in which I intend to examine whether the phenomena be not more naturally explained by the hypothesis of one fluid, than of two. As the fluid in this case issues from a point, it does not exhibit sparks, but a kind of current, that is extremely brilliant.

Must not all these experiments lead us to suspect, that, if glass frequently appear to have no affinity for the electric fluid, it is because its bases, at the time of their uniting in the state of fusion, perfectly neutralise the igneous matter*?

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* Caloric, if you please, as Mr. Libes defines it in the article *Combined Caloric*, which is so interesting and short, that I cannot refrain from copying it. "It is that which intimately combines with the particles of bodies,

Consider, that by an electrification continued for a certain time it is not only reduced to the state of an excellent conductor; as every one, who will subject pointed tubes of glass to a powerful electrification for a time proportionate to the length and thickness of the glass, may convince himself; but that it is capable of supersaturating itself with this fluid, when one side of its surfaces is surrounded with a conducting substance, as metal, water, the hand, &c. Such is the Leyden phial, such the conductors of which I speak here when grasped in the hand, such the glass in this experiment of Mr. Libes, and such the glass balls of experiment 6. Every thing therefore tends to establish an analogy between the nonattraction of glass in the electric state and the neutralization of acids and alkalis, which is sometimes so perfect, that mixtures of two things that burn very powerfully when separate exhibit no sign of causticity. Experiments carefully made by Mr. Lugt, which I shall translate in my Essay on the Leyden phial, prove, that a communication with the ground is not necessary to charge it, but that it is sufficient to establish a sort of circulation between an electrical machine and an insulated phial.

Leyden phial may be charged while insulated.

5th Experiment.

5th experiment.

Electrical chimes.

"Hang three bells on a metallic rod furnished with a hook in the middle to suspend it from the conductor of an electrical machine. Two of these bells are to be fastened by a metallic chain to the end of the rod; the middle bell, and two small bells of metal hanging between it and the bells on each side, are to be fastened to the rod by a silk thread. From the inside of the middle bell depends a chain, which should reach to the floor, or be held in the hand during the experiment. Every thing being thus arranged, electrify the apparatus, and immediately the two little balls of metal will be attracted, each by its corresponding outer bell. Having struck this, they are immediately

---

bodies, and constitutes a part of their substance. In the act of combination it loses its physical properties, and is no longer perceptible by means of the thermometer." Is not this the phlogiston of Stahl under another name?

repelled toward the middle bell. These alternate motions will be repeated as long as the conductor of the machine is electrified."

Confirms the same theory.

These electric chimes form the strongest proof of my inferences. Examine the action of the insulated balls. They are attracted first toward the lateral bells saturated with this fluid: the middle bell, divested of superabundant electricity, and communicating with the ground, attracts toward it the electricity taken from the machine by the balls, and with it the balls themselves. This action may be compared with that of an electrical kite, which, attracting the electric fluid from a charged cloud, conveys it to the ground, if a metallic wire serve it as a conductor. Place an apparatus with two bells in the string of the electrical kite; one communicating with the conducting wire of the kite by means of a metal rod, the other insulated, but having a communication from its inside to the ground like the middle bell of the chimes: the moment the atmosphere has a charge of electricity, the kite will occasion these chimes to act like those of Mr. Libes, and gradually convey the electric fluid from the clouds by their means as well as by a conductor."

Electrical kite

After having given these five experiments as proofs of two opposite actions, Mr. Libes finishes with these reflections on an experiment of Mr. Grey.

"These phenomena of electrical attraction and repulsion led Grey to the idea of imparting to a body by means of electricity an elliptical and at the same time rotatory motion. The following is a description of the apparatus he employed, and the true object of the experiment.

6th Experiment.

6th experiment.

"From the conductor of an electrical machine suspend a ring of stout brass wire about a foot in diameter. Beneath this ring place a circular plate of metal, supported by a stand, so that you can bring it near enough to the ring to prevent the glass balls, that will be mentioned, from slipping between the ring and the plate. On the plate place a ball of glass *blown very thin*, letting one point of its circumference be in contact with the ring. If the apparatus be electrified, we shall see the ball animated at once with a *rotatory* motion, and a circular motion round the plate; and if the experiment be made in the dark, the ball will appear luminous in every point in which it successively touches the ring."

General inferences.

This experiment singularly confirms the inferences I deduce from my experiments, that the nonconducting state of glass is owing only to the powerful affinity of composition of the component particles of good glass; and that when a powerful affinity, or the concurrence of several, presents itself to this substance, the latent igneous fluid is set in motion, and it becomes a conductor, or a body supersaturated with the electric fluid. When it presents itself in this state to a body, that has a strong affinity for the fluid which supersaturates it, it yields it up; but when the action is stronger, and burns, it is decomposed like all other substances; witness the long and continued action on the points of my conductors, and that of the solar rays on the glass of our windows. We see above, that the author requires the ball of glass to be very thin*: this is a necessary condition for producing the rotatory and revolving motion, for every thing made of glass in this state is moved by the slightest electric action; it kindles, as it were, like charcoal before the blowpipe; and being moved in one point, the neighbouring points tend by affinity to carry themselves in succession to the centre of activity. This is the natural way in which I explain these phenomena according to my manner of seeing them; perhaps by the *idola specus* of Bacon. But, si quid novisti rectius istis, candidus imperti—I seek only truth; this alone guides my pen, and in seeking it I write *commenta*, which perhaps time will efface. So many have been made of this kind in all countries, that the ancient Belgium cannot expect to be alone exempt from them.

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* My conductors, when they are of thin glass, become excellent ones in less than a few hundred turns of the machine.

XVII.

der much used in Italy.

and Italy. The English make a
its weight in gold † as a soverei
eases, and fevers attended with
English chemist, informs us, th
that this powder is a triple salt
lime and oxide of antimony. Th

The powder not to be prepared by Dr. Pearson's recipe.

tempted to compose James's pow
son's recipe, have no doubt been
success. They must suppose, th
not to betray a secret from whic
not disclosed the whole truth ||; a
contains besides sulphate of pota
ash.

Mr. Pully, of Naples, has analysed it afresh.

Mr. Pully, a Neapolitan chemi
services to the army of Italy as a
and saltpetre, writes me word, th
powder, of which he procured
quantity to subject it to a strict e
are the particulars of his experim

Warm water dissolved sulphate of potash,

Exp. 1. I took, says Mr. Pull
of which I weighed out 19 decigr.
a few minutes in a little warm di
the water, and obtained from it b
had all the characters of sulphate
solved and treated with barytes,
sulphate of barytes.

Exp. 2. As I perceived, before I decomposed the salt by means of barytes, that the solution contained an excess of free potash, I wished to ascertain, whether this potash did not hold in suspension a little oxide of antimony. In fact having decanted the fluid, to separate it from the sulphate of barytes, I poured into it some sulphuretted hidrogen, which immediately formed golden sulphur of antimony. The free potash, therefore, was combined with a portion of antimony at a minimum of oxidation. Dr. Pearson does not speak of this combination, or of the sulphate of potash.

and some free potash holding oxide of antimony at a minimum.

Exp. 3. I took the James's powder, which had been washed with warm distilled water, and heated it with nitric acid of the strength of 20°. This acid dissolved the phosphate of lime, without attacking the oxide of antimony at a maximum. I separated this oxide from the solution, and poured into the liquor some ammonia, which precipitated the phosphate of lime.

Nitric acid dissolved phosphate of lime.

Exp. 4. I decomposed the phosphate of lime with diluted sulphuric acid, and afterward recomposed it by means of lime water, in order to ascertain the proportions.

Its quantity ascertained.

Exp. 5. I took the oxide of antimony at a maximum of oxidation, and dissolved it in muriatic acid. This solution, treated with sulphuretted hidrogen, produced a hidrosulphuret of antimony, containing more sulphur than kermes mineral, and less than the golden sulphur.

Oxide of antimony treated with muriatic acid.

From these experiments, all the products of which I weighed, the 19 decigr. of James's powder, which I analysed, are composed of

Component parts of James's powder.

	Parts.
Oxide of antimony at a maximum of oxidation	7
Phosphate of lime	4
Sulphate of potash	4·5
Free potash, holding oxide of antimony at a minimum	3·5
	19

To recompose this powder we must take

Method of composing it.

	Parts.
Sulphuret of antimony	4
Calcined phosphate of lime	3
Nitrate of potash	8

These being powdered, mixed, and triturated together, they are put into a crucible, which is to be covered, and exposed to a strong heat. During this operation, the oxigen of the nitric acid, attacking the sulphur of the antimonial sulphuret, converts it into sulphuric acid, which unites with a portion of the potash, and forms sulphate of potash. The remainder of the free potash retains some antimony oxided to a minimum. The white powder, that is left in the crucible, is the same as that sold so dear by the English.

This affords the same products.

Mr. Pully informs me, that he has analysed his powder to compare it with that of Dr. James; and that he has found it to contain the same principles, and in the same proportions.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Grasses.

AT the meeting of this Society on the 11th of March, Dr. Yule read an interesting memoir on the natural order gramineæ, with introductory observations on monocotyledonous plants, in which he contrasted these with the dicotyledonous class from the period of germination to the complete evolution of their stems. The Doctor is to continue the subject in another paper.

Plants with 1 and 2 cotyledons.

Scottish testacea.

Mr. Laskey laid before the Society a list of Scottish testacea, as far as they had fallen under his own observation, with remarks on the new and rare species. Of the genus chiton he enumerated 4 species; of lepas, 3; balanus 6; pholas 4; mya 9, including 3 new species; of ligula (a lately constituted genus) 7 species; solen 6; tellina 15, with a new species named by Colonel Montagu, *t. laskeyi*; cardium 10; mactra 6; donax 3; Venus 23, including 9 new species; chama 1; arca 6; pecten 6; ostrea 1; anomia 4; mytilus 11; pinna 1; nautilus 3; cypræa 1; bulla 13, including 2 new species; voluta 8, 4 of them new; buccinum

num 8; strombus 2; murex 23, comprehending the rare *carinatus*, and 3 new ones; trochus 4; turbo 32, 5 new; helix 17; nerita 7; haliotis 1; patella 11; dentalium 2; serpula 7; vermiculum 3. This is, upon the whole, the most ample catalogue of Scottish testacea hitherto framed, containing 126 species of multivalve and bivalve, and 142 of univalve shells; in all 268 species.

Nondescript fish.

At the same meeting the secretary read a communication from George Montagu, Esq., of Knowel House, giving an account of a nondescript fish, 5 feet long, taken on the coast of Devonshire last summer. It must constitute a new genus, in the apodal order; and Mr. Montagu has bestowed on it the generic name of *ziphotheca*, and the specific one *tetradens*. The communication likewise contained accurate descriptions of four rare species of English fishes; and was accompanied with correct and elegant drawings of these, as well as of the ziphotheca.—At the same time Mr. Montagu presented the Society with copies of his Testacea Britannica, and Supplement, 3 vols. 4to, with coloured plates; and of his Ornithological Dictionary, 2 vols. 8vo.

TO CORRESPONDENTS.

THE learned author of the paper on comets in our last number, p. 206, is Mr. de Luc. His name was accidentally omitted from not being signed to the paper itself, but only to the letter in which it was enclosed.

METEOROLOGICAL JOURNAL

For *MARCH*, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Make
in the STRAND, LONDON.

FEB. Day of	THERMOMETER.				BAROMETER,	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Night.	Day.
25	42	34	45	30	30·26	Fair	Fair
26	34	40	44	34	30·44	Ditto	Ditto
27	42	42	49	34	30·38	Ditto	Ditto
28	38	42	48	38	30·35	Ditto	Ditto
MAR.							
1	43	41	48	39	30·26	Cloudy •	Rain
2	42	38	46	32	30·36	Ditto	Fair
3	34	40	42	38	30·39	Fair	Ditto
4	42	43	46	38	30·16	Ditto	Rain
5	41	38	43	33	30·19	Cloudy	Ditto
6	34	36	40	34	30·26	Ditto	Fair
7	34	36	39	33	30·36	Ditto	Ditto
8	34	37	42	33	30·47	Ditto	Ditto
9	40	48	51	42	30·33	Ditto	Ditto
10	42	36	50	32	30·23	Ditto	Ditto
11	34	38	48	32	30·26	Ditto	Ditto
12	36	38	48	34	30·09	Fair	Ditto
13	36	40	44	37	30·21	Ditto	Ditto
14	40	40	44	38	30·24	Ditto	Ditto
15	38	42	46	34	30·44	Ditto	Cloudy
16	40	44	50	34	30·26	Cloudy	Fair
17	46	50	53	44	30·19	Ditto	Ditto
18	48	46	52	40	30·16	Ditto	Ditto
19	44	46	50	42	30·05	Ditto †	Ditto
20	44	45	50	41	29·97	Fair	Ditto
21	42	46	50	42	30·24	Ditto	Ditto
22	46	50	58	43	30·09	Ditto	Ditto
23	47	52	59	45	29·76	Ditto	Ditto
24	46	45	55	42	29·70	Rain	Ditto
25	44	43	50	38	29·34	Fair	Rain

• At 11 beautiful halo surrounding the Moon.
† Rain at 11 P. M.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXII.

ARTICLE I.

An Account of the Chinese Method of propagating Fruit Trees by Abscission. By Dr. James Howison[*].

SIR,

Chinese method of propagating fruit trees.

THE Chinese, in place of raising fruit trees from seeds or from grafts, as is the custom in Europe, have adopted the following method of increasing them.

They select a tree of that species which they wish to propagate, and fix upon such a branch as will least hurt or disfigure the tree by its removal.

Round this branch, and as near as they can conveniently to its junction with the trunk, they wind a rope, made of straw, besmeared with cow dung, until a ball is formed, five or six times the diameter of the branch. This is intended as a bed into which the young roots may shoot. Having performed this part of the operation, they immediately under the ball divide the bark down to the wood, for nearly two thirds of the circumference of the branch. A cocoa-nut shell or small pot is then hung over the ball, with a hole in its bottom, so small that water put therein will only fall in drops; by this the rope is constantly kept moist, a circumstance necessary to the easy admission of the young roots, and to the supply of nourishment to the branch from this new channel.

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[*] Trans. of the Soc. of Arts, vol. xxv, p. 14.

During three succeeding weeks, nothing farther is required, except supplying the vessels with water. At the expiration of that period one third of the remaining bark is cut, and the former incision is carried considerably deeper into the wood, as by this time it is expected that some roots have struck into the rope, and are giving their assistance in support of the branch.

After a similar period the same operation is repeated, and in about two months from the commencement of the process, the roots may generally be seen intersecting each other on the surface of the ball, which is a sign, that they are sufficiently advanced to admit of the separation of the branch from the tree. This is best done by sawing it off at the incision, care being taken that the rope, which by this time is nearly rotten, is not shaken off by the motion. The branch is then planted as a young tree.

In Europe a longer time required.

It appears probable, that, to succeed with this operation in Europe, a longer period would be necessary, vegetation being much slower in Europe than in India, the chief field of my experiments. I am, however, of opinion, from some trials which I have lately made on cherry trees, that an additional month would be adequate to make up for the deficiency of climate.

Advantages of this mode.

The advantages to be derived from this method are, that a further growth of three or four years is sufficient, when the branches are of any considerable size, to bring them to their full bearing state; whereas, even in India, eight or ten years are necessary with most kinds of fruit trees, if raised from the seed.

When at Prince of Wales's Island, I had an opportunity of seeing this proved by experiment. Some orange trees had been raised by a gentleman, from seeds sown in 1786, which had not borne fruit in 1795, while branches taken off by the Chinese mode in 1791, had produced two plentiful crops.

Whether

Applicable to timber trees;

Whether forest trees might be propagated in Europe in the same manner, I have not had experience sufficient to form a judgment: if it should be found practicable, the advantages from it would be great, as the infancy of trees would, by this means, be done away, a period which, from the slowness of their growth, and the accidents to which they are liable, is the most discouraging to planters.

and natives of warm climates.

The adoption of this method will, at all events, be of great use in multiplying such plants as are natives of warmer climates, the seeds of which do not arrive here at sufficient maturity, to render them prolific.

These branches bear well.

I have frequently remarked, that such branches of fruit trees, as were under the operation of abscission during the time of bearing, were more laden with fruit than any other part of the tree. It appeared to me probable, that this arose from a plethora, or fulness, occasioned by the communication between the trunk and branches through the descending vessels being cut off by the division of the bark, while that by the ligneous circles or ascending vessels, being deeper seated, remains*. The same reasoning accounts for fruit trees producing a greater crop than usual, on being stripped of their leaves, most of the ascending juices being thrown off by them in perspiration, or expended in their nourishment, for we find that bleeding trees cease to give out their juices after they have put forth their leaves †.

Stripping fruit trees of their leaves.

I have observed, that the roots from a branch under the operation of abscission were uniformly much longer in shooting into the rope when the tree was in leaf, than the contrary; hence, the spring season appears most proper for performing this operation.

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* The circumstances attending the Chinese method of propagating fruit trees appear a strong confirmation of Mr. Bonnet's opinion, that plants, as well as animals, have a regular circulation of their fluids.

† Marsden, in his History of Sumatra, page 119, says, "The natives, when they would force a tree that is backward to produce fruit, strip it of its leaves, by which means the nutritive juices are reserved for that important use, and the blossoms soon show themselves in abundance."

It

Reference to the Engraving, Plat
Method of propagating Fru

Explanation of the plate.

A. The tree on which the ope

B. The straw rope wound in the tree.

C. The cocoa-nut shell, or v which gradually drops thence on

D. Another branch of the san E, rooted in the straw rope o planting out, has been separated

F. The vessel suspended fron which the ball has been supplied

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II.

Description of a Gauge or M invented by Mr. JAMES BROA

SIR,

Standing timber liable to be estimated very erroneously,

THE Instrument I send her of standing timber, and will, I ceedingly useful to all gentleme generally the case. When an estate is sold on which the timber is to be valued, I believe, there is no other way in general use of finding the girth of a tree (which, being squared and multiplied by its length, gives the contents) than by actually getting up to the middle, where the girth is usually taken, with a ladder or otherwise: a method which is very troublesome and expensive where the quantity is large. The seller has, therefore, no way, but at an enormous expense, of finding the real contents of what he has to offer, and as the buyer, if a dealer, from his knowledge is able to form a more accurate judgment, it often happens, that the seller sustains much loss. *I have known it exceed 50 per cent.* Having some time ago a large quantity to survey, I thought it possible to invent an instrument, which would obviate this inconvenience, and which might be sold at a low price, be correct in its work, quick in execution, and such as any capacity might use. I likewise thought it might be so contrived, as to make such an allowance for bark, as should be agreed on. The instrument I send you possesses all these qualifications, and is susceptible of several improvements, of which I was not aware when I made it, which I will point out at the end of my letter.

unless measured at great expense.

It is well known, that the diameter and circumference of circles are in a certain proportion to each other, and that double the diameter gives double the circumference. The allowance for bark is usually one inch in thirteen, that is, if the greater circumference of a tree with the bark on is found to be thirteen inches, it is supposed it would be only 12 inches if the bark was taken off.

Allowance for bark.

The instrument is composed of two straight pieces of well seasoned deal, about thirteen feet long, joined together by a pin going through them, on which they are movable; but neither the length nor thickness is of any particular consequence, as, by following the directions hereafter given, they may be made of any size. A little way from the larger end is a brass limb, I call the index, on which are engraven figures denoting the quarter-girth in feet and inches. To use this instrument, it is only necessary to take hold of the large end, and apply the other to that part of the tree where you wish to know the girth, opening it so wide as just to touch

Measuring instrument described.

at the same time both sides of it, without straining it, keeping the graduated side of the index uppermost, on which the girth will be shown, after allowing for the bark, by the inner edge of the brass on the right hand leg. An operation so easy and simple, that a person of the meanest capacity might measure a great number of trees in a day.

Instrument for measuring the height.

For taking the height of a tree, I would recommend deal rods of seven feet long, made so as to fit into ferrils at the end of each other, tapering all the way in the same manner as a fishing rod. A set of five of them, with feet marked on them, would enable a man quickly to measure a tree of more than forty feet high, as he would be able to reach himself about seven feet.

Improvements, that might be made in it.

The improvements it is capable of are, making a joint in the arch or scale, to enable it to shut up (when the legs are closed) towards the centre, which would make it easier to carry. Secondly, as it sometimes happens, that standing timber is sold without any allowance for bark, and at other times with a less allowance than one inch in thirteen, two other scales on the index might be added in such cases, one without any allowance, and the other to allow as might be agreed on. I would have added these, but thought the Society would rather see it in the state in which it has been tried on a large survey, as any artist can with great ease add whatever scale he pleases. The present scale allows one inch in thirteen for bark, and is calculated on the following data. The diameter of a circle the quarter circumference of which is 26 inches, is $33\frac{10}{100}$ inches. The diameter of a circle, the quarter girth of which is $6\frac{1}{2}$ inches, is $8\frac{27}{100}$ inches. To graduate the scale, the instrument is opened so as to take in at the small end between the touching points $8\frac{27}{100}$ inches, and a mark is made on the arch to denote 6 inches quarter girth: it is then opened so as to take in $33\frac{10}{100}$ inches, and another mark is then made on the arch, to denote two feet quarter girth; (these marks are made close to the inner edge of the brass on the right hand limb): the space between them is then divided into eighteen parts, which represent inches, and are again divided into halves, for half inches; if any notice is to be taken of quarter inches, the eye will easily make a farther decision.

Nicholson's Philos. Journal Vol. XXIII Pl. X. p. 32

Mr. J. Broad's Timber Gauge

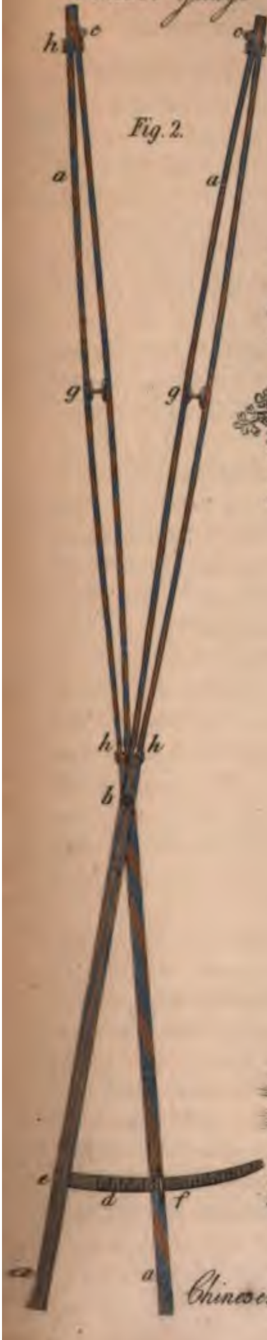

Fig. 2.

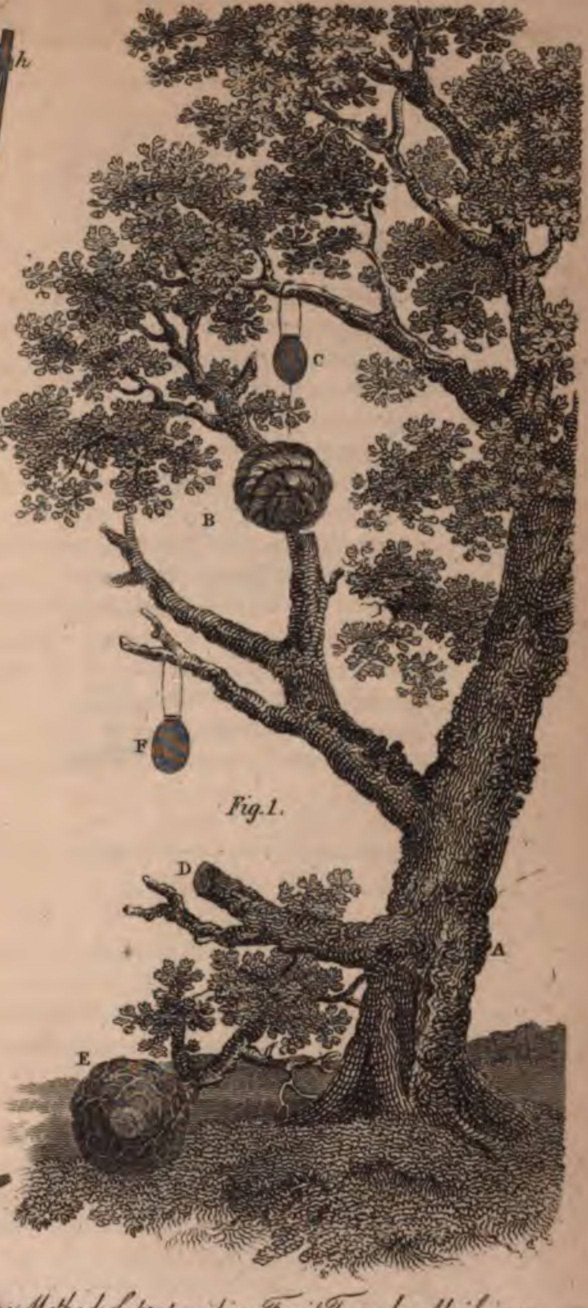

Fig. 1.

Chinese Method of propagating Fruit Trees by Abciſsion.

R. Pattle sc.

THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

I beg leave to add, that it is not my intention to make any for sale.

I am, Sir,
Your obedient Servant,
JAMES BROAD.

Reference to the Engraving of Mr. James Broad's Machine for measuring standing Timber. Plate IX, Fig. 2.

Explanation of the plate.

Fig. 2. *aaaa* Two long pieces of well-seasoned wood, joined near the middle by a pin *b* going through them, forming an axis on which they move. *cc* Two pieces of brass screwed near their upper ends, on the sides opposite to each other, and projecting over to form the measuring points. *d* The index fastened to one of the pieces of wood at *e*, and moving freely under a small bar at *f*. *gg* Screws with nuts, placed in the middle of the long slits of the two arms, to wedge them open, whereby the vibration is destroyed, and the arms, though light, are rendered stiff. *hhhh* Screws and nuts to prevent the arms from splitting.

A certificate from Mr. J. Wilkins, carpenter, of Sandy Lane, dated May 4, 1805, stated, that he had used the instrument invented by Mr. J. Broad, for measuring timber standing, and that he believes it to be a correct and valuable one.

III.

Report of a Committee appointed by the Bath and West of England Society, to investigate the Claim of the Right Hon. Lord Somerville to a Premium "for the greatest Number and most profitable Sort of Sheep.*"

YOUR Committee report, that the claim is founded upon facts, as under:

Lord Somerville's stock of sheep,

About the year 1800, Lord Somerville's stock (as stated by him in his Memorial to the Society) consisted of forty-five ewes of the long-woolled sort. Finding these annually degenerating, and also becoming annually less profitable, he changed them at the above-mentioned period for one hundred and fifty Ryeland ewes.

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* Bath Society's Papers, vol. X, p. 71.

Changed for Ryeland.

Improved.

In the first year, though the winter was severe, the ewes supported themselves tolerably well, and the lambs were in very good order at weaning time. In their future growth, as wethers and store ewes, they far exceeded in weight their parent stock. One lot of the wethers sold as high as 3*l.* each, and were fed upon grass and hay only.

Mixed with Merino.

In the following year, Lord Somerville brought from Spain some rams and ewes of the Merino breed. These rams, in each subsequent year, have been, and now continue to be, put to ewes of the South-Down and Ryeland breed; from each of which crosses a valuable species of sheep has been obtained, both in fleece and carcase; the relative value of which has been detailed by his lordship in his memorial of 1802, the substance of which is, that

Profit of South-Downs;

South-Down store-ewes at 3 *lb.* per fleece, and at 1*s.* 10*d.* per *lb.*, will pay 5*s.* 6*d.* per fleece; which, at 6½ per acre in good upland pasture for seven months, and five months in turnips at 14 or 15 per acre, will pay 38*s.* or 40*s.* per acre.

of Ryelands;

Ryeland store-ewes 2¼ *lb.* per fleece, at 2*s.* 2*d.* per *lb.* untrinded, nine sheep per acre, and turnips as above, will pay 2*l.* 3*s.* 10½*d.* per acre.

of South-Down and Merino;

South-Down and Merino ewes of the half-breed, at 4 *lb.* per fleece clean washed, and 3*s.* per *lb.*, will amount to 12*s.* per fleece; which, at 7½ per acre for seven months, amount to 4*l.* 10*s.* per acre for the pasture land, with turnips as above for winter keep.

of Ryeland and Merino;

Ryeland and Merino ewes of the half blood at 10 per acre for seven months, and turnips as above, at 3¼ *lb.* per fleece, and 3*s.* 2*d.* per *lb.*, amount to 6*l.* 10*s.* 5*d.* per acre.

Pure Merino.

The pure Merino fleeces never sold at less than one guinea each; the average weight of which has been more than 6 *lb.* each in the yolk; and on the above allowance of pasture for seven months, and turnips as above in aid of that pasture, the return will amount to ten guineas per acre.

Size of the farm.

The size of the farm in Lord Somerville's occupation is four hundred and sixty acres, eighty-five of which are a dairy unfit for sheep, except for a few couples in the spring. These sheep have been depastured as under:

Feed for sheep.

56 Acres one and two years old clovers, indifferent keep, some worn-out ley.
85 Acres marsh and capital pasture.
35 ditto upland summer pasture.
5 ditto just taken in hand, foul.
7 Keep upon thirty acres of water-meadow for six weeks in the spring, equal to one
—— fourth the number of acres, or seven.

Other stock on the same ground.

Total - 188 acres; beside the run of thirty-three acres of turnips. But it is to be observed, that in the same ground sixteen plough oxen occasionally, but twelve constantly, were depastured; four horses occasionally, four cows constantly, till the after-grass: to this is to be added the run of yearling calves, and a large stock of pigs; and that the green crops of the spring and summer, 1803, were unusually deficient in these districts. The sheep stock amounted to

Sheep stock.

302 lambs,
783 store sheep,
——
Total - 1085;

Produce.

The produce of them as follows:

	£	s.	d.
Wool, 12 packs, 1 score - -	*£446	0	0
216 store-sheep sold - - - - -	409	3	0
132 fat sheep ditto and used - -	238	16	2
Letting of rams - - - - - - - -	524	10	0
	£1618	9	2

No sheep older than four teeth fatted.

N. B. No sheep have ever been kept fattening older than four teeth. The fat sheep were all sold after being sheared, and the price of mutton, from the deficiency of keep, was not more than 4½*d.* per *lb.* at one part of the season.

J. F. LUTTRELL.
JOHN BRICKDALE.
W. JONES

Fitzhead, June 2, 1804.

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* Five hundred fleeces of the above, at 15*s.* 8*d.* per fleece; amounted to 392*l.*

Omitted to take into account keep* of sheep in Sir J. Trevelyan's park, 18*l.* and about six tons of hay, at two guineas per ton, 12*l.* 12*s.* Per contra creditor, for turnips given to two bulls, 4*l.* 12*s.* leaving a balance of 26*l.* to be deducted from the above

£1618 9 2
26 0 0

£1592 9 2

IV.

On the Advantages of the Use of Oxen and Neat Cattle in Husbandry. By Lord Somerville †.

SIR,

London, *Dec.* 8, 1804.

YOU will have the goodness to express to the Bath Society my regret, that I cannot attend its anniversary meeting, as was my intention.

Change of sheep stock.

On the subject of my claim to the premium for a change of sheep stock, &c.‡, I have only to observe, that it was made at a period the most unfavourable to stock, when distress for keep of all sorts was greater than I have ever known; and that whether the premium be adjudged or not to me, I shall ever consider the favourable reception it met from the committee to which it was referred, the able essays which it produced, and the recommendation which this committee, numerously attended, has unanimously given to this general meeting that it should be awarded, together with the proofs since produced, as decisive on this most important question.

Return of labour with oxen.

The return of my year's labour with oxen was made out for the year 1803, because the year 1804 not being expired, to have made it without the amendment, since resolved on, would have been a palpable errour. I now comply with the repeated and earnest wishes of the society, in presenting this statement for its inspection.

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* Occasioned by the total failure of grass in the marsh, from the severe draught in September.

† Bath Society's papers, vol. x. p. 56. ‡ See the preceding article.

Unprepared as I was, it would have been impossible to have done it with that degree of accuracy, which I shall adhere to in any statement of serious import to the public; but the rules which govern the proceedings of the Bath Society allowing me time to examine my own books, and to obtain replies to certain needful questions, I have great pleasure in stating, that the following report is now presented to the society, in substance, I trust, correct. In that part of the statement, which reduces the hauling and carting of manure to be equal to a given number of acres ploughed, I have profited by the kind assistance of Mr. Paul, and Mr. Gordon Grey.

Correct.

No land ploughed with horses, save part of one acre, as a trial.

Work done by twelve oxen in one year.

	No of acres.
Forty-four acres of ley ground broke up -	44
Fifty ditto of spring corn, two earths, scarified and dragged, equal to 1½ earth more - - - - - - - -	170
Sixty ditto of turnips, at three earths, cultivated, or scarified and dragged, equal to 1½ earth - - - - - - - -	271
Twenty-two ditto pease, at one earth, and broad-cast dragged, part of it drilled	26
Fifty-seven acres of wheat, 32 acres on one earth, once dragged, and twenty-five of heavier land, twice ploughed and twice dragged - - - - - - - -	123
Twenty-nine ditto of ley ground, broke up to December 1st - - - - -	29
Three ditto of beans, seven ditto winter vetches, two ditto potatoes, cultivating, dragging, &c. - - - - - -	17
Lime carried and "butted"* over the land, 160 butts, equal to two acres work, being a large proportion of 1920 hhds. or 9600 bushels of lime - - -	120 Hauling 125 Butting*

Carried forwards 925

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* Butting. The lime being mixed with the forehead or headlands, round the edges of the field, is thence carried in implements called butts, bodily over the land—a laborious but good management.

Brought forward 925

Twenty-five acres of the turnip land dunged, equal to 30½ - - - - - 30½

Total 955½

Hay-harvest, corn-harvest, corn to market, hauling of timber, &c. not easily calculated, but still to be allowed for, probably amounting to

Allowing for the three-year-old steers ninety acres, and eight half-days work of two two-year-old bulls, worked occasionally with the steers for the purpose of keeping them quiet; allowing also as above the hay-harvest &c. not brought to account; it will appear, that the labour of the twelve oxen throughout the year will amount to, if not exceed one thousand acres.

Average of labour in a day.

The average amount of our labour is two acres of ley ground per day, and fallowing and stirring more than two acres; but the second cross ploughing, or earth, somewhat less. Our teams consist of four oxen, a man, and a boy, to each double-furrow plough, and to each four-wheel waggon. Our ploughing in general very deep, and our fields small, not exceeding 4½ acres each on the average. These two circumstances are to be duly considered as adding materially to the labour. The working stock consists of sixteen steers and oxen, two bulls, and three light horses, *viz.* six five-year-old oxen, six four-year-old steers, and four three-year-old ditto. They are fresh growing stock, and are regularly turned out to graze after the barley-sowing, at six years old. The whole object aimed at is to carry on our course of crops on the most speedy and vigorous system, but without injury to the growth of the stock.

Working stock.

This labour not injurious.

It is evident, that my labour, severe as it long has been, cannot be found to injure the health or growth of the stock: the exhibition of my oxen annually, within ten months grazing from the time they are turned out of work, will fully exemplify this important fact. This was in great part my object in establishing the *Barbican* cattle-shew; and if I may be allowed to say so, the effect already produced, more especially in countries where oxen were held in

Barbican cattle shew.

in disrepute as animals of labour, has exceeded my most sanguine expectations. That our crops are worked in so expeditiously as to amaze those who contend for horse-labour only, cannot be denied; in proof of which, fifty-seven acres of wheat were this autumn ploughed, sown, and manured in a complete manner, according to the usage of the country, within three weeks, although the weather was unfavourable, and the land worked close and heavy. The last nine acres were ploughed, sown, dragged, and harrowed, in one day. In obedience to the wishes of the society I present them with this statement; but I wave all claim to a premium. If, however, in the ordinary course of business, any man in this kingdom shall be found to have done more at a less cost, I shall consider myself as having trespassed unworthily on the notice of this society. It is fitting to add, that in twenty years labour I have not lost one ox or steer, or ever broke a yoke or pair, by sickness, death, or accident. And I may farther add, that, so far from incurring any loss of value from working cattle after their full growth, as is supposed to be the case with horses, amounting to 25 per cent or more; my own experience, and the concurring opinion of the committee sent to examine our stock in the month of June last, warrant me in declaring, that working-cattle, from three to six years of age, do actually gain at the rate of 20 per cent yearly; the loss in my own case, in twenty years, being nothing!

Superiority of oxen.

Not one beast lost in twenty years.

Average gain of working cattle.

The premium now in question, having distinctly waved my claim to it, will probably be awarded to Mr. Billingsley; and it gives me sincere pleasure, that it should be bestowed on him. He has been a most strenuous and successful advocate for the labour of oxen, and is well aware of its extreme importance. Mr. Billingsley has accomplished a measure hitherto untried, namely, to set out his ploughing by the acre, and to apply one team of oxen, full grown, with two to assist, in all eight oxen, to constant plough-labour, every day in the year that it was possible for them to work. I consider the attempt of such consequence to the landed interest, so momentous an illustration of the powers of these superior animals in labour, that I beg leave

Mr. Billingsley sets out his ploughing by the acre.

leave here to offer him my sincere thanks; and I have the honour to be,

With all respect to the Society, &c.

SOMERVILLE.

V.

Practical Statement on the foregoing Subject, with Claim of Premium. *By* John Billingsley, Esq. *

Work done by a team of six oxen in eleven months.

THE claimant states, that his servant Esau Green has on a farm of eight hundred acres, the soil of which is of a middling texture, ploughed and harrowed, with a team of six oxen and a double-furrow plough, the following acres of land, *statute-measure*, between the 1st day of January and the 1st of December, 1804, *viz*.

Acres.	
56	of oat stubble.
62	of turnips for oats.
68	of ley.

Statement accurate.

The claimant farther states, that no possible errour can have crept into this statement. No other team being employed in the tillage of this farm, and the ploughman being paid by *the acre*. Nor can any doubt arise respecting the size of the acres, as it is well known, that lands newly enclosed are set out *statute-measure*.

Oxen employed.

The oxen employed were home-bred, of the long-horned race, and were purchased last year of one of the claimant's tenants, at the price of 14*l.* each. Four of them were six years old, and four four years old.

Not injured by their work.

Though eight oxen were kept, six only were worked at a time. The other two were changed as occasion required, at the will of the ploughman. These oxen are in no respect injured by their labour, and are now in good working order.

Price of labour.

The ploughman and driver were paid 1*s.* 4*d.* per acre for ploughing, and 6*d.* per acre for harrowing; and in this was included all necessary attendance on cattle at all times and seasons.

The depth of ploughing from 3 to 5 inches.
The breadth of ditto from 7 to 10 inches.

Harrowing.

As the harrowing was all performed with six oxen, drawing very heavy and long-tined harrows, (provincially called drags) and in many fields two bouts in a place, it will not be unfair to estimate *two* harrowings as equal to *one* ploughing; and in proof of this comparison it may be observed, that the double plough will turn two acres and a half in eight hours, which are half as much as six oxen can harrow in the same time.

Statement of expence.

Presuming that no solid objection can be brought to the foregoing statement, it may be satisfactory to the society to see the debtor and creditor sides of the account, methodically arranged, so as to ascertain the cost both of the ploughing and harrowing per acre, *statute-measure*.

Dr.

Eight Working Oxen, from Jan. 1st to Dec. 1st, 1804.

	£.	s.	d.
To 24 lb. of hay per week, consumed between Jan. 1 and May 12, when they were turned to grass, at 2*l.* 10*s.* - - - - -	47	10	0

	£	s.	d.
Brought forward	47	10	0
To 24 week's keep at grass, at 3s. each ox -	28	16	0
To 6 tons of hay, between Oct. 1 and Dec. 1, when at grass - - - - -	15	0	0
To repair of yokes and bows - - -	0	18	0
To wear of plough and dressing shares, mending chains, &c. - - - - - -	4	13	6
To cash paid Esau Green for ploughing 385 acres in eleven months, at 1s. 4d. - - -	25	13	4
To cash for harrowing 291 acres, at 6d. - -	7	5	6
	£129	16	4

N. B. This price per acre includes driver and attendance.

			Cr.
By 385 acres ploughed, at 4s. 10½d. - -	93	8	10
By 291 acres harrowed, at 2s. 6d. - -	36	7	6
	£129	16	4

Saving.

If this had been let by hire, it would not have been taken by any neighbouring farmer at less than 8s. per acre ploughing, and 4s. per acre harrowing.

Proportion of work.

If the harrowing of *two* acres be admitted as equal to ploughing *one*, it follows, that the work done by these oxen, (caparisoned in the old-fashioned way with yokes and bows) is equal to the ploughing of 530 acres in eleven months, or 578 acres in a year.

Earnings of the man and boy.

It may naturally be supposed, that on so elevated and exposed a hill as that of Mendip many interruptions to tillage work must occur in the course of a year, notwithstanding which the man's earnings (driver included) amount to 14s. per week nearly; to which must be added sundry work in the time of harvest, (after his day's work at ploughing) and other occasional labour, amounting to the average of 2s. per week, constituting altogether the receipt of 13s. per week for himself, and 3s. per week for his boy.

In conducting this experiment the claimant has sedulously guarded his mind against all bias and partiality, being desirous of getting at the truth by the most accurate investigation; and he has no doubt, that the statement of work performed under the direction of a noble lord (the rival candidate for this premium) will lead to the same conclusions, and rescue this most useful animal from that degraded state of inferiority, in which he has unjustly been classed.

At Midsummer last the claimant had not the most distant idea of starting as a candidate; and since that time he has in no way whatever stimulated the ploughman to *extraordinary* exertion.

Advantage of ploughing by the piece.

In letting his tillage labour by contract he has not departed from the uniform practice of twenty-five years. A practice originating from an idea, that the best method of making servants laborious and honest is to make it their interest to be so.

Encouraging however as these facts must be to the use of oxen in this department of husbandry, he does not venture to say, that they will answer in all countries, or on all soils.

On some soils horses may be nearly as cheap as oxen.

On all light sandy soils, such as Norfolk, Suffolk, &c. single ploughs of different constructions, drawn by two horses without a driver, may in cheapness of execution nearly approach the double-furrow drawn by four oxen. But on all level soils unincumbered with stones, and where good pasture may be found for *summer*, and good hay for *winter* keeping, oxen with the double-plough may in his opinion be considered as justly entitled to a preference.

JOHN BILLINGSLEY.

Bath, *Dec. 6th*, 1804.

VI.

On the Conversion of French Weights into English, in a Letter from MR. JOHN FAREY.

To Mr. NICHOLSON.

SIR,

Tables of reference should be correct.

THE importance of having tables of constant reference as free as possible from errours will I hope plead my excuse for troubling you on this occasion, and obtain insertion for this in your Journal, in order to point out two very erroneous columns in the table, which you gave in your 4to Journal, vol. I, p. 332, for reducing *grammes* of the new French system of weights into English grains, and vol. II, p. 284, for reducing English grains into grammes.

The French gramme erroneously given.

The errours here alluded to seem to have originated at p. 199 of your first volume, where one gramme is stated to be 22·966 English grains, instead of 15·457, which results from multiplying 18·841, the number of French grains, by ·8204.

Corrected in the British Cyclopædia.

Since the above determination of the *gramme*, the completion of the meridional measurements has introduced a small correction, which occasions, I suppose, the gramme to be stated at 15·444, in the article GRAMME in your new *British Cyclopædia*, which is the proper reciprocal of ·06475, stated therein, under the article WEIGHT, as equivalent to 1 grain troy; but in your 4to Journal vol. II, p. 284, the whole column is calculated from ·04354 instead of the above.

Corrections of other weights.

Permit me here farther to remark on the article WEIGHT, in the last part of the *British Cyclopædia* just published, that 1 pound troy is 288 scruples; and not 283; that 1 scruple is 1·295 grammes and not 1·395; 1 ounce avoirdupois is 28·328 grammes, and not 28·32; 1 dram is 27·34375 grains, and not 37·975: and 1 dram is also 1·7705 grammes and not 1·81. The English troy pound contains or equals 7021 Paris grains nearly, and not 702; the English avoirdupoise pound is 8532·5 French grains, and not 8538, according to the table on the opposite page thereto: the first column of which table is inconsistent with (as not a true reciprocal to) the second column; for 1 French grain is 0·8204 English grains, as is rightly stated on the preceding page, and not ·8203; 10000 French grains are 8204 English grains, and not 8233, as there stated.

As unfortunately the conclusion of the British Cyclopædia prevents the *errata* being there given, I have set them down here, and am,

SIR,

Your obedient humble Servant,

JOHN FAREY.

12, *Upper Crown Street, Westminster.*
12th January 1809.

VII.

VII.

Account of an Improvement in Tram Plates for Carriages on Rail Roads. By Mr. CHARLES LE CAAN, *of Llanelly in Wales**.

SIR,

I HAVE forwarded to the Society of Arts &c. a specimen of my new method of laying rails, or tram-plates, on such a plan as has met the entire approbation of those who have seen it, and are acquainted with the principle on which such roads should be formed. Rail roads are daily increasing, from the great advantage they afford to manufactories connected with mines and minerals, and particularly to collieries. They also promote Agriculture, by occasioning lime to be procured from places almost inaccessible by any other means, or whence it could be otherwise brought on moderate terms.

Rail roads much increasing from their advantages.

I flatter myself, that every improvement on this system will be of national importance. The honour I received last year from the Society of Arts &c. has stimulated me to submit the present subject to their consideration.

I have also sent a drawing of my method of laying the tram-plates, with an estimate of the saving that will arise to the public, by adopting the said method, with necessary remarks on the principle on which it is founded. The leading rail or tram-plate has neither tenon nor mortise over the plug. The stop-plate terminates the specimen, which stop-plate should go in with some degree of tightness when laid for actual use, but in the present case that force is not necessary, as the wooden blocks, by a carriage of upwards of 200 miles, may in some small degree be misplaced. I hope any impediment of that nature will be rectified or allowed for. I wish it to be understood, that a stop-rail is intended to be placed at every 30 yards, at which distance any repairs may be made within ten minutes, which by the present mode frequently occupies more than twice that time, exclusive of disturbing in some measure the line of

Improved rails or tram-plates.

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* Transactions of the Society of Arts, vol. xxv, p. 87. Twenty guineas were voted to Mr. le Caan for this improvement.

road. By my method, the plates have a certain degree of play, which is absolutely necessary to avoid that breakage, which too frequently takes place when they are fixed with nails and plugs.

The plates which I send have been fixed in stone blocks, and are nearly as rough as when taken from the sand. *If I* am favoured with any mark of the society's approbation, I shall hold myself bound to transmit such farther communications on this subject as may be required by them, or any person desirous of adopting my plan.

I am, Sir,
Your most obedient Servant,
CHARLES LE CAAN.

Llanelly, Carmarthenshire,
May 12, 1806.

SIR,

Advantages of them.

I HAVE considered the improvement made by you in the specimen exhibited of a new design of a tram-plate, and am of opinion, that much advantage may be derived to tram roads by the adoption of your plan, in preventing the temptation of stealing the wrought iron nails, with which the plates are usually fastened, and by facilitating the operation of laying down new tram roads, and repairing of old ones.

I am, Sir,
Your most humble Servant,
J. VANCOUVER.

Llangennech Park, April 2, 1806.
To Mr. le Caan.

SIR,

Advantages.

SEVERAL tram-plates on your new method of fixing without either nail or plug have been cast under my immediate inspection, at Stradey furnace. The same may be made with as much ease as any others now in use, and I conceive they will obviate the many impediments that arise from the irregularity of driving the nails. I have no doubt from my observation of yours, but that they will answer extremely well, and prove less expensive.

I am, Sir,
Your obedient Servant,
J. LEWIS.

Stradey Furnace, April 18, 1806.
To Mr. le Caan.

Further testimonies.

Certificates were also received from Mr. R. Jones of Swansea, agent to General Ward's colliery; from Mr. James Barnes, who formed the Myther and Carmarthenshire rail road; and Mr. Edward Martin, of Morristen, an eminent colliery, surveyor and planner of rail roads, all testifying the advantage of Mr. le Caan's invention.

Reference to the Engraving of Mr. Charles le Caan's *improved Tram-Plates for Rail-roads. Pl.* X, *Fig.* 1, 2, 3, 4.

Description of the tram-plates and method of making rail roads.

The tram-plates, fig. 1 and 2, are fastened by means of a tenon and mortise A B, each having a correspondent bevel, just sufficient to keep the end from rising up, so that the head of one plate confines the end of the other; by this means, the workmen are obliged to form their road in right lines, and maintain perfect levels, as the mortise and tenon confines them to the required exactness necessary to make a perfect road: curves or any given segment may be formed with the same nicety, by having two bevel rails or plates made for such purposes.

Fig. 2, A side view or longitudinal section of the two plates placed on their stone blocks or sleepers C D, shows two plugs in dotted lines, one bevel, the other perpendicular, cast in the stop-rail or plate, which is so called as it prevents the others from moving, and when taken up releases all those between the stop-plates; 25 yards of rail road made with these plates, may be taken up and replaced within ten minutes. The plugs in dotted lines are shown in their proper positions within the sleepers E F G.

The

Description of the tram-plates and method of making rail roads.

The usual length of a tram-plate is three feet, the flanch or outside edge H, about one inch and half high, the sole or bed I, from three inches and a half to four inches broad, and three fourths of an inch thick; but these dimensions may be varied according to circumstances. The most approved weight has been 14 pounds to the foot, or 42 pounds to the plate, the ends from which the plugs project, and to which the tenons and mortises fasten, should be one fourth of an inch thicker than the other part of the plate.

Fig 3. A B show the under part of the tenon and mortise, and the form of one of the sloping or bevel plugs.

The diameter of the plug near the shoulder is one inch and three quarters, reducing to one inch, its length two inches and a half, forming an angle of eight degrees, the plate from which it projects is counter sunk, so that the shoulder of the plug may not receive any sharp pressure or prevent the plate from having a perfect bearing. There is a small groove in the whole length of the exterior of each plug, to admit a wire to pass to its extremity, to draw the plug out if broken by any accident, also to admit the expansion of water, in case of severe frost.

The blocks or sleepers, E F G, on which the tram-plates are placed, should by no means be less than 120 pounds each in weight, but should be heavier on some kinds of ground: the depth of the hole for the plug should be three inches, and worked according to the inclination of the plug, for which purpose the stone-mason should have a standard cast-iron gauge; there should be projections, K, cast with the flanch or outside edge of the tram-plate, as shown at fig. 1, to make the plates lie firm on their sleepers.

Fig. 4 is a section of one of the ends of a tram-plate, in which H shows the flanch or upright edge, I the flat part or sole on which the wheels of the waggons run, D one of the plugs, K the projection behind the flanch to make the plate lie firm on the blocks.

GENERAL OBSERVATIONS.

Advantages of this compared with the common mode.

The advantages of laying plates on the above principle are obvious: the blocks being put in their places never sink below their intended level, the act of driving either nail or plug, (which requires a considerable degree of force, and too frequently destroys the level of the road) being here unnecessary. In the common mode of making rail-roads, from the irregularity of nails, particularly in forming their heads, few can be driven exactly even with the plate, and they are perpetually obstructing the passage of the waggon; the workmen frequently not proportioning their holes and plugs to the hole in the block also occasions considerable breakage; the exertion necessary to fix a rail or plate completely is great, and numbers of plates, particularly when the iron is short or brittle, are broken near the mortices by missing the stroke of the hammer, which must be used with great force.

Advantage gained in laying my Tram-Plates in Comparison with other Modes

Saving in expense.

	£.	s.	d.
Nails used in a mile, 3520 of 3 in the pound, at 4*d.* per lb. - - - - - -	19	11	0
Nails lost or defective, computed at per mile	1	0	0
Plugs with their loss - - - -	6	5	0
By breakage of rails, average from experience	7	10	0
Lessened by labour in block laying, calculated at only two pence per yard - - -	14	13	4
By breakage of blocks - - - -	1	0	0
	£49	19	4

This calculation does not take in annual loss of nails, and breakage of blocks, which is considerable.

VIII. *Observations*

VIII.

Observations on the Use of Acid Fumigations in purifying the Air and stopping the Progress of Contagion, and the most simple Means of completely obtaining this Effect. Extracted from the Correspondence of Mr. Guyton-Morveau *.

Contagious disease stopped by acid fumigations.

I CANNOT yet give you any particulars of the disease, that prevailed at Pethiviers at the end of last summer; though several persons, who have seen the reports of the officers of health, have assured me, as I informed you at the time, that acid fumigations had evidently put an end to it. I shall say nothing of what may have prevented the result from being published, or why it has not even been mentioned in the newspapers. You will scarcely believe, that indifference could be carried so far on a subject of such importance. No doubt it would be otherwise, if government appeared to set any value on the discovery: but there are so many petty interests, that oppose its utility being made known; and it is so much more easy for a man to throw a veil over the efficacy of the means, than to confess himself chargeable of having been ignorant of them, or of having neglected to apply them to the benefit of mankind!

Obstructions to improvements.

Infectious diseases arise without direct contagion.

I have been informed, that a physician in considerable repute asserted a few days ago, that these fumigations were inefficacious against diseases not communicated by infection. It would not have been amiss to have asked him, whether he thought balls and bayonets conveyed any infectious principles: or whether he did not know, that, whenever a great number of wounded persons were conveyed to an hospital, a disease did not soon break out, that attacked the nurses, the medical attendants, and often a whole town. For, as I have said on a former occasion, whatever be the first cause or the nature of a disease, that affects several individuals at the same time, it necessarily produces an hospital fever, which ultimately occasions more ravage than the original disorder; and it is precisely against this scourge, that mine-

Hospital fever.

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* Annales de Chimie, vol. xlvi, p. 114.

ral acid fumigations afford a certain preservative, a remedy the efficacy of which can no longer be disputed.

Testimonies collected by Mr. Guyton.

The more secret motives there are, to mislead opinion on this subject, the more incumbent on me it is, to collect such facts as testimonies, as may produce conviction. I have at present some to communicate to you, which cannot fail to appear capable of making an impression on all those, who are not misled by prejudice; and I will add some remarks on the bottles for destroying contagion, which are not yet sufficiently known, and the method of employing them for the use of a large hospital, so as to have always at hand, without trouble or expense, a certain preservative, and a powerful stimulus, the strength of which may be regulated at pleasure.

Letter from Dr. Mojon.

At Genoa acid fumigations employed against

Dr. Mojon, professor of chemistry and member of the Medical Society at Genoa, who had already furnished me with some valuable information respecting the use of mineral acid fumigations in that city, which makes a part of the additions to the second edition of my treatise, has since addressed to me the following letter.

an epidemic fever,

"As soon as we perceived the progress of the epidemic fever, recourse was had to the acid fumigations in our churches, hospitals, lazarettoes, prisons, and barracks, and in the chambers of several sick persons.

in a church against noxious effluvia,

"On the 20th of March, 1800, I was sent for to St. Andrew's church, where two grave-diggers had dropped dead, just as they were going down into a vault. I found the church offensive with putrid exhalations; and immediately directed the vault to be walled up. After causing the windows to be shut, I placed in the midst of the church a large earthen vessel containing six pounds of common salt, and three of sulphuric acid. Some lighted faggots were placed round the vessel, to accelerate the extrication of the vapours, which ceased at the expiration of two hours, and then the windows were opened. The noisome smell was completely gone, and the church was used as before, without any thing offensive being perceived.

in a very large church,

"I observed the same effect still more strikingly, when I employed oxigenized muriatic acid fumigations in the largest edifices, particularly in St. Dominic's church, where the air was so noisome, and so loaded with putrid emanations, that its stench was perceptible to some distance and in the adjacent houses. For this fumigation I employed eight pounds of salt, four pounds of sulphuric acid, and a pound and half of black oxide of manganese.

and in the chambers of the sick.

"To purify the air of close and habited houses, I preferred fumigations with nitric acid, which were equally successful, destroying the contagious miasmata without occasioning the least inconvenience to the sick. There is no instance of any one having caught infection from a sick person, where these fumigations were constantly used.

Acetic acid used as a preservative.

"To secure myself from the effects of the putrid and contagious exhalations, to which I was daily exposed, I made use of no other prophylactic than a small phial of acetic acid (radical vinegar), which I held to my nose, and by its means I had the happiness of escaping the infection during the whole continuance of the epidemic."

Effects of Fumigations with oxigenized Muriatic Acid, as reported by Mr. Fleury, Officer of Health in the Navy.

Oximuriatic acid fumigations in dysentery,

Mr. J. A. Fleury, in his Essay on the Dysentery, presented to the School of Physic, January the 4th, 1803, after classing frequent fumigations with oxigenized muriatic acid among the general means of regimen and renovation of the air, adds what follows as a note in support of this precept.

and against the hospital sore.

"After the signature of the preliminaries of peace and the treaty of Amiens, a great number of French prisoners landed at Cherbourg. The sick were carried to the naval hospital, of which I had the charge. Several were afflicted with the ulcer called the hospital sore, to remedy which all the resources of physic and surgery were employed in vain. The fumigations of oxigenized muriatic acid, employed habitually to correct the air, were particularly directed to these ulcers; and soon the contagion, which imparted to other ulcers and recent wounds the same character, was stopped, and I had the satisfaction to see cures gradually effected, which I had before attempted in vain. Though these fumigations were made in wards full of patients, from the impossibility of avoiding it, and this rendered their more frequent employment necessary, I never found one person complain of them, or sensibly incommoded by them. It was not the same with those, who entered when the vapour was already in a state of expansion; as it brought on these a violent cough, and obliged them to quit the ward immediately."

On the Preparatiom and Use of the Phials for removing Contagion.

Phials for preventing and removing contagion.

The phials for preventing and removing contagion, which I have mentioned in my treatise, have become pretty common, since they have been kept ready prepared in the shop of Mr. Boullay. In fact it is not easy to conceive any thing more simple in its preparation, more convenient for use, and less expensive, considering the property this composition has of retaining its virtue a long time. I have one of these phials prepared near twelve years ago, and it cannot now be uncorked, without the persons present being immediately sensible of the oxigenized muriatic acid gas, though I have used it on various occasions, and no addition has been made to it since it was first prepared. This is the property of what I call *extemporaneous* oxigenized muriatic acid, because it is made in an instant, without the aid of fire or any apparatus for distillation, in short by simple mixture.

The ingredients must be accurately proportioned.

However easy this process is, certain proportions are nevertheless necessary to be observed, in order that the evolution of the gas may be sufficiently abundant to be efficacious, yet not so rapid as to burst the vessel: and it is obvious, that the proportions do not depend simply on the relative quantities of the acids in this case, but still much more on their degree of concentration.

Several persons, having seen the stopples of these phials rise spontaneously the moment they opened them, were alarmed, and mentioned it to Mr. Boulay; who accordingly thought it necessary to dilute the mixture, that it might be less active. Soon after this I was informed, that phials recently bought of him scarcely made any impression on the nose when opened under it; and some were brought me, from which I found in fact but a slight smell of common muriatic acid. On my representing this immediately to Mr. Boulay, he at once made his phials as strong as they ought to be; and I know, that those he has since sold were well prepared. This however suggested to me, that it might be proper, to determine the process with more precision than I had hitherto done, I mean with regard to weight and measure, in order to have a liquid at all times manageable, and of a permanent degree of activity. I shall proceed therefore to give the receipt with all its particulars.

The phials described.

The phials to be made *portable* should scarcely exceed in capacity 4½ centilitres [1½ oz.], or 45 cubic centimetres [about 2¼ cubic inches]. This is the measure of those prepared by Mr. Boulay, who puts them into a case of hard wood, commonly of box, neatly made. The cap of this case screws on. It is unnecessary to say, that the stopple of the phial must be of glass, and ground to fit perfectly tight.

Method of preparing them

Having selected a phial, it is to be measured. Suppose its capacity to be just 45 cub. cent. [1½ oz.], 3 gr. [46·32 grains] of black oxide of manganese are to be put into it, powdered, but not finely, and sifted only through a hair sieve.

To these add 7·5 cub. centim. [about ⅕ of a cubic inch] or ¾ of a centilitre [¼ of an oz.] of pure nitric acid of the specific gravity of 1·4 (about 39° of Baumé's areometer), and an equal measure of muriatic acid of the specific gravity of 1·134 (about 17° of Baumé's areometer).

The stopple being put in, the process is finished.

They must not be more than one third full.

It is to be observed, that about two thirds of the capacity of the phial will thus remain empty. This is an essential condition, without which it is impossible to confine the gas. Having once exceeded this proportion in a very strong flint-glass phial, that would hold 4 decil. [13 oz.], I saw the stopple, which weighed 122 gram. [1884 grs.], driven out to such a height, that in falling it broke the phial. But it is easy to prevent all accidents, by keeping within the limits here assigned.

It will not be improper, to add to the instructions for preparing these phials the manner of using them; for things acquire value only by the skill with which they are used.

Manner of using them.

In the first place it is to be observed, that the intention of the screw is chiefly to bring the cap of the case to the proper point for confining the stopple of the phial; which might otherwise be raised up by the expansion of the vapour, and allow the acid to escape; so that, if you were to endeavour to turn the screw as far as it would go, or even if you were to turn it with too much force, you would necessarily split the cap, or even crack the neck of the phial, which would be still more dangerous, if it were not immediately perceived.

Caution.

I have observed, that the first thing done by those who are unacquainted with the nature of acid gasses is, to apply the phial to the nose, as they would a smelling bottle; whence they feel an irritation so much the more painful, because it is more quickly carried to its maximum. It is necessary therefore, to inform such persons, that the phial for destroying contagion should not be brought near the nose: on the contrary, it should be kept at a distance from it when it is unstopped; and when it begins to make an impression on the olfactory nerves, it is time to stop it again, unless it be required to render the fumigation efficacious in a given space, as when the object is to purify a chamber rendered noisome by putrid effluvia. In this the phial should be placed on a table, and left unstopped for some minutes. With these precautions we may obtain all its good effects, without experiencing the least inconvenience.

So much for the use of the portable phials: you will soon see, that I have carried my views much farther respecting the advantages to be derived from the preparation they contain.

Permanent Apparatus for destroying Contagion in Hospitals, public Places, &c.

Permanent apparatus for large buildings.

It is with some unwillingness I employ the word *apparatus*, which may perhaps be sufficient to frighten many persons,

sons, though in fact it is applied only to a vessel kept ready to be opened when necessary, and which in this view might be called a *box of salubrity;* but the name is of little importance, and I leave it to custom to settle this, so the thing itself be adopted.

In reciting the numerous experiments I made in one of our hottest summers on considerable bodies of air contaminated by sanious putrefaction, I announced, that I took the precaution frequently to leave open in my laboratory a very large phial, containing the mixture above mentioned for the extemporaneous production of oxigenized muriatic acid gas. This phial, which had since been neglected, having fallen in my way a few days ago, I was surprised, on taking out the stopple, at the strength of the gas it still furnished after the expiration of two years. This convinced me, that the mixture, kept in sufficient quantity in large vessels, would supply the place of all fumigations to destroy contagion, and answer the purpose as effectually, without trouble, expense, or inconvenience, and without its being necessary to renew the preparation till after a considerable time, even in cases where it would be most frequently necessary to give issue to the gas.

The mixture continues to yield oximuriatic acid gas a long while.

It is obvious, that the capacity of the vessel must be proportionate to the extent of the space to be purified, and its aperture sufficiently large, to give instantly the volume of gas required; that is to say, so as to extend to every part without too powerfully affecting those who are nearest to it. Lastly, the gas must be so confined, that it cannot burst out, or even escape imperceptibly: in a word, so that it will only diffuse itself around when we wish it, cease to be diffused at pleasure, and remain for months without its presence being suspected.

Necessary conditions.

All these conditions are easily obtainable for the largest hospital ward by the means I am about to describe.

Easily obtainable.

Take one of those very thick flint glass tumbers, which are common in the shops, of 11 or 12 cent. [4·3 or 4·7 inches] high, and 10 [3·9 in.] in diameter; holding about 7 decil. [1·47 wine pint]. Grind the edge so that it may be closed by a piece of plate glass. Cement the bottom of the tumbler into a piece of wood, which is to be made fast by

Apparatus described.

by sliding horizontally into grooves at the bottom of two uprights. To these uprights a cap must be fixed, through which passes a screw, to raise or lower the stopple by means of a nut in a sliding cap, to which the piece of plate glass is to be cemented.

An inspection of the figures, *Pl.* X, *Figs.* 5 and 6, will show the form and dimensions of every part of this little apparatus, which should be wholly of wood, without iron or any other metal, and the construction of which requires nothing extraordinary or at all expensive.

A a small square of wood, into which are fixed the two uprights, B B.

C a glass tumbler, cemented into a little movable piece of wood, *d*, which is secured by a groove in each of the uprights.

E a wooden screw, passing through the upper cross piece F, and carrying the movable cross piece G, which slides on the two uprights by means of a groove at each end.

H the plate of glass performing the office of a stopple, and cemented to the under side of the movable cross piece.

The vessel being thus arranged, and its capacity being we will suppose 7 decil. [near 1½ pint], pour in first a decilitre [3·369 oz.] of nitric acid, of the strength mentioned above, and then an equal quantity of muriatic acid; add 40 gram. [618 grs.] of black oxide of manganese powdered; and immediately close the vessel by screwing down the stopple. These proportions are assigned from the necessity of leaving at least two thirds of the capacity of the tumbler empty.

If the contagion were considerable, or if the foci from which it issued were sufficiently numerous, to renew it in a short time, it would be advisable to have two or three such apparatuses in the length of the ward.

In large or very foul places two or more necessary.

In a place less extensive than I have hitherto supposed, in a ward for instance containing but ten or twelve beds, or in a public room where the air is vitiated only by a temporary accumulation of animal effluvia, we might employ, instead of one of these tumblers, a wide mouthed stopple bottle, such as the glassmen sell for the use of chemists. Their

In some cases a large wide mouthed phial sufficient.

Their capacity is commonly about 40 or 45 centil. [14 or 15 oz.]; and their stopples, which are made to fit tight, are 3 cent. [1·18 inch] or more in diameter.

It is obvious, that by putting into one of these phials 6 centil. [2·22 oz.] of each of the two acids, and 24 gram. [370 grs.] of black oxide of manganese, we shall readily obtain a reservoir of gas for destroying infection. The only thing to be apprehended, against which the apparatus above described is secure, is that the stopple, being secured only by its weight and the friction of the neck, should be forced out by the elasticity of the gas: but this may be prevented by loading the stopple with a heavy cap of lead.

Method of using them.

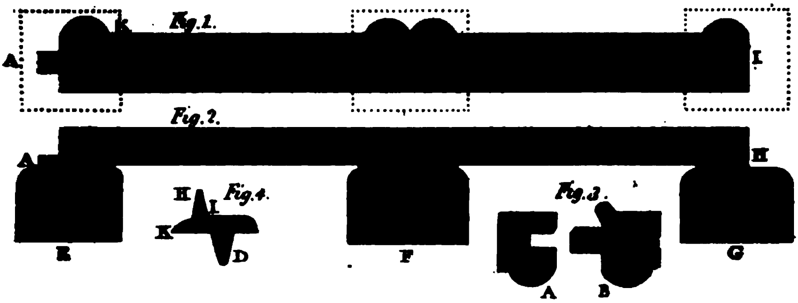
No farther direction need be given respecting the mode of using these reservoirs of gas for destroying contagion, than to open them when it is deemed requisite, and to close them as soon as the persons nearest at hand begin to be affected by them. After this we may rely on the spontaneous expansion of the portion set at liberty. The effect will be such, that, if the vessel have remained open only four or five minutes, those who come into the room an hour after by the remotest door will immediately perceive, that oxigenized muriatic acid gas has been extricated.

Advantages of the oximuriatic acid.

You will be of opinion assuredly, that of all the processes adopted for fumigation this is the most simple, the least liable to accident, and the best adapted to common use: and when you consider, that the gas it sets in action is acknowledged to be the most efficacious, even by those who have appeared to dread its activity, because they were unacquainted with the means of moderating it at will: and lastly, if you adopt the decided opinion of several professional gentlemen, that this gas has the property of exciting the vital powers above all other acid gasses: you will perhaps think with me, that when the petty passions are exhausted by struggling against conviction, the oxigenized muriatic acid gas will be adopted in preference as the primary antidote to contagion, and its extemporaneous preparation will be the most common resource in the regimen of health.

IX. *Account*

Mr. Charles Le Caan's Tram Plates.



Guyton's Permanent Apparatus for destroying Contagion

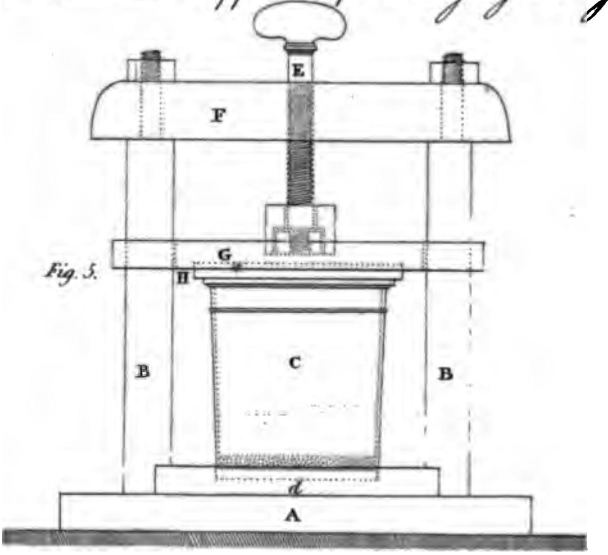


Fig. 5.

Plan

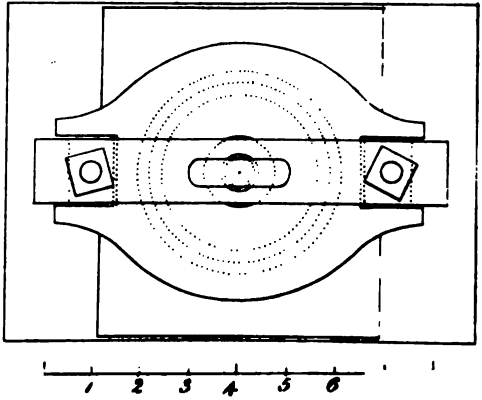


Fig. 6

IX.

Account of a Well for preserving and filtering Rain-water for domestic Purposes, where a Supply of Spring-water was not easily to be obtained. Communicated by J. R. Gowen, *Esq.*

To Mr. NICHOLSON,

SIR,

Well for filtrating rain water.

YOU may perhaps deem the following account of a filtering rain-water well, which has been successfully tried here by the Earl of Caernarvon, not undeserving of notice in your valuable Journal. His lordship has lately erected, upon a dry gravelly eminence in his park, an ornamental circular building, consisting of a room and open colonnades above, and apartments for cottagers upon the basement floor. Considerable discussion arose upon the mode of supplying them with water, from the great depth to which it was necessary to sink, in order to obtain an effective well. My friend, Mr. John Loat, builder, of Clapham, who had furnished the plan for the construction of the dome roof, mentioned to me a contrivance of his father's to meet a similar difficulty, which had been attended with invariable success, and Lord Caernarvon immediately determined upon carrying it into execution.

Difficulty of supplying water on an eminence,

obviated by collecting rain-water in one well and filtrating it into another.

Following Mr. Loat's instructions, we sunk two wells, 30 feet deep by 4 feet diameter each, which for greater perspicuity I shall call No. 1 and 2. They are a trifling distance asunder, and were carefully clayed, to prevent percolation into the surrounding soil, and lined with bricks in the usual manner. A well secured communication was made between the two wells, by a small leaden pipe inserted two feet from the bottom. All the pipes from the roof were directed into No. 1; and an oak floor, bored full of small holes, and supported upon posts, was laid in at No. 2, just above the pipe of communication. Upon this floor was first placed a stratum of well washed coarse gravel, then one of finer, next a stratum of coarse sand, and finally one of the finest sand we could procure, making altogether two feet in thickness of silicious substances. The water, which is received into No. 1, passes through the leaden pipe into No. 2, and filtrates by ascent through the strata of sand and gravel, the space below the level of the oak floor in both wells, acting as a cespool, receives all sediment. The pump is of course affixed in the filtering well. Both wells are covered up, but plenty of air is admitted to them, through apertures made for this purpose.

Advantages of this plan.

You will immediately perceive, that the merit of this plan consists altogether in the filtration by ascent, with a competent space under the apparatus. The interstices of the sand are thus never clogged, and its power is preserved unimpaired for an indefinite period. The well fully answers its intended purpose, and the water is altogether excellent. I have been tempted to submit this statement to you from a persuasion, that there are few houses, which may not be made in this manner to supply excellent water in sufficient quantity for domestic consumption; and that situations abound, where the filtrating well may be resorted to with equal comfort and advantage.

I am, Sir,

Your obedient humble Servant,

J. R. GOWEN.

Highclere, Newbury, Berks,
April 1, 1809.

X.

An Inquiry into the Structure of Seeds, and especially into the true Nature of that Part called by Gærtner the Vitellus. By JAMES EDWARD SMITH, *M.D. F.R.S. P.L.S.*[*]

Gærtner first named and defined the *vitellus*.

GÆRTNER, so justly celebrated for his anatomical and physiological inquiries into the nature of seeds in general, and for his particular illustration of one thousand different kinds, claims the merit of first giving a name and definition to a part called by him the *vitellus*, which, though not entirely unobserved by preceding philosophers, had received no particular description or explanation. Before we enter upon the investigation of this organ, it is necessary to consider the structure and functions of the parts of a seed in general; and this it will be best to do physiologically.

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[*] Trans. of the Linnean Society, vol. xx, p. 204.

Germination of seeds requires moisture, heat, and air.

Three agents are necessary to the germination of seeds,—moisture, heat, and air. A seed committed to the ground absorbs, through the vessels of its base, the juices of the soil, or any other moisture that comes in its way; while it receives, throughout its whole substance, a definite portion of heat, some seeds requiring a greater share of the latter, for the purposes of vegetation, than others. Moisture and heat however are not of themselves sufficient to cause the germination of seeds. It has long been known, that air is equally necessary; and modern chemists have ascertained oxigen gas to be the particular ingredient of the atmospheric air which is requisite, and which is absorbed by seeds, in the moments of incipient germination, from or through the surrounding soil. Thus the bulk of the seed is increased, and its vital principle stimulated. It bursts its immediate integument, or *testa*, and in the first place sends forth the radicle, or young root, into the ground.

Integument.

Radicle.

This part being, as Dr. Darwin well observes, most susceptible of the stimulus of moisture, elongates itself in the direction in which it meets with this stimulus; and descending into the earth, while it fixes the infant plant, assumes its own proper function of imbibing nourishment for the future support of that plant.

Albumen.

But before any supplies can be thus obtained, considerable demands are made, even by the root itself; and not only an evolution of parts, but likewise an increase of bulk, takes place in the young vegetable. For this necessary purpose a store is prepared in the *albumen*, a substance either constituting a separate body by itself, as in grasses, corn, palms, &c., which, from a hard, dry, and tasteless mass, changes, by the action of water and oxigen, into a milky or saccharine fluid; or the same substance is lodged in, or united with, the bulk of another part, next to be mentioned, the cotyledon, or, as they are generally of the plural number, Cotyledons.

As the root is the part stimulated by moisture, the cotyledons appear to be most stimulated by air, and they consequently raise themselves, for the most part, out of the ground in order to receive it, in the form of seminal leaves, well known to perform, for a time, the functions of real leaves, and even, by the action of light, to assume their green colour. The *albumen* cannot be said to be stimulated, or acted upon as a living body, by the air or gas, which only produces chemical changes in it; and the destination of this substance being soon accomplished, it disappears by absorption. Not so the other parts of the seed, one of which becomes the still descending root, the other the nurse, or, if we may say so, the foster brother of the young ascending plant, which last originates from the extremity of the embryo opposite to the root, but always, like that, most intimately connected with the cotyledons. These indeed, sooner or later, wither away; when the acquisition of real and more ample foliage renders them superfluous, or no longer necessary. But all cotyledons do not ascend out of the earth, nor assume any of those functions of leaves in which light is concerned. In the horse chesnut, the *cyamus nelumbo*, the *tropæolum majus*, and some other plants, they always remain buried, no doubt acted upon by the air or gas alone. Even in plants of the same natural order, *papilionaceæ*, some, as *lupinus*, raise their cotyledons into the air and light, in the form of very conspicuous green seed-leaves; while others, as *lathyrus*, retain them under ground, concealed in the black skin of the seed, quite out of the reach of every ray of the latter. In these we know a farinaceous *albumen* is lodged, whether they rise into the light or not; and the closest analogy leads us to conclude, that their functions are otherwise similar, which can only be with respect to air.

Not indispensable.

Even cotyledons however are not indispensably requisite to a seed, though the albumen appears to be, in some form or other, necessary to all seeds. Not to mention the tribes of vegetables allowed or guessed to be without cotyledons, and thence, for systematical convenience, denominated acotyledonous; all, who have sufficiently considered the matter, know that in those called monocotyledonous, what is vulgarly taken

Absent in plants called monocotyledonous.

for the cotyledon is really an albumen, a part fundamentally distinct in functions from what is proper to a cotyledon. Thus even so conspicuous a family of plants as the *orchideæ*, which the faithful Jussieu confesses were only presumed from analogy to be monocotyledonous, or, as he guardedly expresses it, to have "a single-lobed *corculum*," have been shown by Mr. Salisbury, in the eighth volume of our Transactions, the only person I believe who has well examined their germination, to have in fact an albumen, but no cotyledon at all. Nor does such ambiguity or uncertainty belong to this family alone. Many plants are presumed to be monocotyledonous, chiefly because they grow in the water; and it is much to be regretted, that this fundamental principle of all natural systems should in many cases be so ill-established, and very often so extremely difficult to detect or to determine; which happens in general where its help is most wanted, as I shall presently endeavour to show; but I must first speak of the more immediate object of the present essay.

Vitellus.

Its characters according to Gærtner.

Gærtner asserts the *vitellus* of seeds to be "distinct from "the cotyledons as well as from the albumen, and, for the "most part, situate between the latter and the embryo." He considers as its principal diagnostics the three following characters: "1st, that it is most closely connected with the "embryo, so as not to be separable from it without injury "to its own substance: 2dly, that notwithstanding this in- "timate connection, it never rises out of the integuments "of the seed, as the cotyledons usually do, in germination, "so as to become a seminal leaf, but, rather like the al- "bumen, its whole substance is destroyed by the seedling "plant, and converted into its own nourishment: and 3dly, "that if the albumen be likewise present, the *vitellus* is al- "ways situate betwixt that and the embryo, in such a man- "ner, however, that it may be separated from the albumen "with great ease and without injury." For which reasons this able writer considers the organ in question as "allied "on the one hand to the albumen, on the other to the co- "tyledons," but truly distinct in nature from both. He proceeds to observe, that "it is of all the internal parts of "a seed the most singular, and by far the most unfre- "quent."

Now,

These do not hold.

Now, to consider all these points separately, in the 1st place the *vitellus* is not more closely connected with the embryo than the greater part of cotyledons are; according to the figures and descriptions of Gærtner himself, the fidelity of which must be evident to any one in the habit of using his book, and especially to those who will take the trouble of comparing a few of them with the seeds to which they refer, while in the earliest stage of germination, at which time the relative connection of the parts is best ascertained. 2dly, That the *vitellus* never rises out of the ground, is a circumstance common to it with many cotyledons, allowed to be such by Gærtner, as in the leguminous plants, and others already mentioned. 3dly, That the *vitellus* is situate between the albumen (if the latter be present as a separate organ) and the embryo, is only a necessary consequence of the more intimate connection between it and the latter than either of them has with any other part, which is also precisely true of the cotyledons and embryo, as above mentioned.

It does not differ from the subterraneous cotyledons.

For these reasons I presume the *vitellus* to differ in no respect from the subterraneous cotyledons already described; and that its office is to perform the necessary functions relative to air or oxigen, till the leaves come forth and assume those functions, in greater perfection, with the cooperation of light. This seems more satisfactory than the opinion of Gærtner, that the organ under consideration affords nourishment to the embryo; because this is abundantly supplied by the copious *albumen* of a multitude of seeds, the *vitellus* of which is very inconsiderable, as grasses; and because it is unphilosophical to recur to two causes, when one is evidently sufficient. In fact, the *vitellus*, as far as I can observe, only dwindles away when the leaves unfold, exactly as happens to the subterraneous cotyledons. The same thing very often takes place as speedily in those which rise out of the ground; the existence of the latter appearing to be prolonged in some instances, merely by their nearer approach to the nature of leaves, as in umbelliferous and cruciform plants. The difference of duration is still more evident, and more instructive as to our present purpose, in the leguminous family, between such cotyledons as rise above the ground, like lupines, and those which remain buried, like vetches, the latter decaying as quickly as any supposed *vitellus* can do. In grasses the scale, taken by Gærtner for a *vitellus*, is mostly so thin and unsubstantial, as not possibly to contain any material portion of nourishment; but its expanded figure is very well calculated, like that of the leaves, for functions analogous to vegetable respiration, and its whole aspect conveys the idea of a primary or subterraneous leaf, quickly rendered superfluous by the production of real leaves, which, as well as the radicle, are probably, in the first stage of their evolution, fed by the abundant juices of the albumen. It appears, that the pretended *vitellus* is not necessary to all plants furnished with this distinct kind of albumen. The palms and *orchideæ* prove to be destitute of it. On the other hand, I can find no instance of a supposed *vitellus*, and a real cotyledon or cotyledons, in the same plant. What Gærtner terms the cotyledons of *rhizophora*, in his *tab*. 45, appears to me to be the *plumula*; and in his descriptions of some of the *scitamineæ*, he evidently takes the latter for a cotyledon.

The scale of grasses analogous to leaves in its functions.

Vitellus never present with a cotyledon.

By understanding the *vitellus* as a cotyledon, all ambiguity respecting the component parts of any seed is removed. When the cotyledons are two or more, the only question is, whether the albuminous matter is lodged in their substance, or whether it forms a separate organ. When the embryo is accompanied by a simple undivided organ or seed-lobe, we know it to be a cotyledon by its strict union, or even partial incorporation, with the embryo, as in *zamia**; whereas the pure separate albumen of the true palms has, as in every other instance, no more connection with the embryo, according to Gærtner's just remark, than is absolutely necessary: and moreover evinces its true nature by the chemical alteration, and speedy absorption, of its whole substance. The cotyledon, as I consider it, of *zamia*, as in numerous

Considered as a cotyledon.

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* Mr. R. Brown, who has observed the germination of a large species of *zamia* in New Holland, assures me that he found no such incorporation of the parts in question as Gærtner has represented in his *t*. 3, and that the structure and evolution of every part bore an exact resemblance to *cycas* as described by Mr. Aubert du Petit Thouars.

numerous parallel instances, shrivels and shrinks indeed considerably, from the absorption of its albuminous contents by the vegetating embryo, but does not disappear, leaving only a skin behind, like the albumen of grasses or corn, because that part of its substance, which is destined to perform the office, essential to a cotyledon, concerning air, merely decays when its end is answered.

Difference in the albumen.

It may further be observed upon this subject, that the albuminous matter of seeds with two or more cotyledons is commonly of an oily nature, while those with one cotyledon, or none at all, have a more farinaceous, or even stony, albumen. Still the latter changes to a milky or oily fluid, previous to its absorption. When the vital principle of a seed is extinct, its albuminous oil becomes rancid, and, even in seeds that retain life, is liable to suffer some deterioration by keeping. Hence, as Darwin observes, gardeners preserve melon and cucumber seeds, perhaps for years, that the plants they produce may be less luxuriant, in consequence of being starved at their first germination; for any injury to the cotyledons, even after they begin to rise above ground, is found to cramp the subsequent growth of the plant.

Oil of the cotyledons.

The oil of the cotyledons has been usually supposed a protection to their internal parts, I presume against wet; but this purpose it by no means does or can answer, for all seeds readily absorb moisture whenever they meet with it, and, if likewise exposed to the action of oxigen, they vegetate, in whatever situation they may otherwise happen to be. I suspect moreover that the oily and mucilaginous fluids of seeds in general, before they perform their office in germination, all previously become milky, and often saccharine, from the actions of water and oxigen. It might be worth while to inquire, whether exposure of such seeds as are most prone to turn rancid, to a quantity of oxigen, would tend to preserve them. It is, I believe, found, that the admission of some atmospheric air is necessary to the preservation of many seeds. The primary cause of decay therefore in seeds spoiled by keeping may originate, not, as I have supposed, in the extinction of their vital principle, but in the corruption of their albuminous oils; and this is strengthened

Would oxigen preserve seeds prone to turn rancid?

strengthened by the experiments of the French chemists, whose applications may much more readily be supposed to correct and restore the albuminous juices, than to bring the dead to life.

The albumen, when not a separate body, lodged in the cotyledons.

This idea of the albuminous matter, whether oily, mucilaginous, or farinaceous, being, when not a distinct and separate body, always lodged in the cotyledons, throws additional light on the nature of the last mentioned parts, and in a very beautiful manner confirms their analogy with leaves. The discoveries of Mr. Knight have proved, that the nutritious fluid or sap of plants is carried into the leaves, in order to be there acted upon by air, light, heat, and moisture. After these agents have produced their effects, the fluids are sent back, through the returning vessels, into the branch or stem, to furnish matter of increase to the whole vegetable body. The chemical experiments, of Dr. Priestley more especially, confirm this, by teaching us, that carbonic acid gas is absorbed by leaves in the day time through their upper surface, and decomposed by them, its carbon being added to the sap, and its oxigen emitted by the under surface. In the dark, leaves are found to absorb oxigen.

Chemical process in germination.

Let us apply all this to the germination of seeds. The oxigen, known, as I have already said, to be necessary to this process, being conveyed to the seed in its dark subterraneous situation, is absorbed by its cotyledons, already stored, from the constitution of the parent plant during their formation, with albuminous matter abounding with the carbonic principle. The chemical action, of the oxigen on this albuminous substance renders the latter a more or less saccharine, and, with the addition of the imbibed moisture, a milky fluid, fit to be transmitted, through the returning vessels of the cotyledons, into the stem of the embryo, especially as all these important parts have already begun to swell by the absorption of moisture assisted by warmth. Hence we see why light is found hurtful to incipient germination, and why carbonic acid gas may be given out by seeds at that period. We perceive also why the outside of seeds is so commonly dark coloured, or even black, as in *canna*, *afzelia*, and others, it being the only part of the vegetable body, as far as I recollect, that is ever positively black, except per-

Light hurtful to incipient germination.

haps the skins of some fruits. It is, moreover, evident, that all the indispensable functions of the cotyledons are best performed under ground, and that when they rise into the air and light, it is not till after their primary destination is fulfilled, and then because, being fundamentally of the nature of leaves, they are also capable, in most instances, of assuming their functions with respect to light. It is highly worthy of notice, that, in consequence of the original position of the cotyledons in all seeds, the oxigen gas must always be imbibed by their under side, that very same part which in leaves gives out this kind of gas during the day, and probably absorbs it during the night. It would have evinced a strange contrariety in the constitutions of two organs otherwise so analogous, I mean the leaves and cotyledons, if the *upper* surface of the latter, while in the unexpanded seed, had been presented to receive the oxigen gas.

Stalk of the embryo may perform the function of a cotyledon, where the latter is wanting.

Where there is a separate albumen, without any perceptible cotyledons, it is probable that the stalk of the embryo may answer the necessary purpose; just as the stems of leafless plants must be presumed to perform the usual chemical functions of leaves, though we cannot ascertain in what direction the different airs are imbibed or discharged, there being no decided upper or under surface in such stems, any more than in ensiform leaves. Such, however, are rare exceptions, which if not, as yet, found to throw any new light on the subject, certainly do not overturn any important part of the above hypothesis. That some part, immediately connected with the embryo, must be stimulated in order to excite the germination of a seed, this phænomenon being dependent on the vital principle, is evident. I conceive that, when present, the cotyledon or cotyledons are themselves stimulated by the oxigen gas, or rather by the heat which chemists inform us is produced by the absorption of this gas, so as to set their fluids in motion, and thus to propel the young root and rising *plumula*. But when the cotyledons are wanting, the embryo may very well be conceived capable of sufficient action, to imbibe for itself the juices of a distinct albumen, already become milky and saccharine by the reception of oxigen and moisture; by which merely chemical process, as in barley, so considerable a degree of heat is evolved, as must very powerfully excite the vital principle of the budding vegetable. In the few cases where one or more cotyledons and a distinct albumen are together present, it does not seem necessary, that the gas should act through the former upon the albumen, the two organs being but little connected, and its operation on the latter being independent of all vital or organic laws; but either the gas itself, or the heat produced, may very well so stimulate the vital principle of the cotyledons, as to propel their fluids into the embryo, and assist germination. This opinion is the more probable, as these fluids must be supposed more truly of the nature of sap, and more immediately fit for the use of the infant plant, than the liquor of the albumen. However this may be, the existence of a cotyledon or cotyledons, together with a separate albumen, in seeds, seems to me so unusual, as not to occasion much difficulty, and I would define a cotyledon to be a vital organ, capable, as such, of being stimulated by oxigen, heat, or both, for the propulsion of its contents; while such an albumen is merely a repository of nutritious vegetable matter, subject to the laws of chemistry alone, and only passively resigning those contents to the absorbing powers of the embryo, to which it is attached.

Definitions of the cotyledon and albumen.

Arrangement of plants by their cotyledons.

I must now, under the impression of what has just been advanced, return to the arrangement of plants by their cotyledons.

Those with two or more.

Plants in general are dicotyledonous, having a pair of these organs, which commonly rise out of the ground; but if they do not, it appears, from the consideration of the leguminous tribe, that such a difference could scarcely serve for a generic distinction, much less for that of a class or order. It also appears, that, if the number of cotyledons exceeds two, as in *pinus* and a few other instances, the difference is of little or no use for systematical purposes, and of no physiological importance whatever. The cotyledons of *pinus* all present their backs to receive the oxigen.

Monocotyledonous.

Some plants appear to be really furnished with one simple cotyledon, as *zamia*, and according to Gærtner's figures and descriptions, the true *scitamineæ*, as *amomum* (his *zingiber*), *alpinia*, &c.; while *canna* seems to have no cotyledon, but only an albumen. Can this be true? and if so, what is the value of such a distinction in a natural classification? The *liliaceæ*, *palmæ*, and now the *orchideæ*, are acknowledged to be acotyledonous, having only an albumen; while the grasses, so nearly allied to them, have one cotyledon, for I presume their scale must be admitted as such. Gærtner's phrase of *embryo monocotyledoneus* applied to these last-mentioned families may occasion a mistake, which would be avoided by the term *embryo simplex*, or *indivisus*, expressing his idea of the simple figure appropriate to this part in such plants, but which does not prevent its upper extremity being strictly analogous to the *plumula* of the *dicotyledones*. It seems to me therefore, that this learned writer is mistaken in saying the monocotyledonous plants never have any *plumula*. They have not indeed that feather-like configuration in the ascending point of their embryo, which gave rise to the name, but the organ so called is, and must be, present. To dispute about the term is as little to the purpose as to contend, that the *orchideæ* have no *pollen*, because it is not of a powdery appearance.

Ferns. From Mr. Lindsay's account of the germination of ferns in our 2d volume, this family must be deemed monocotyledonous. Their germination seems at first analogous to that of mosses, as given by Hedwig in his *Theoria*, but the numerous and branched cotyledons of the latter overset all analogy, and indeed all classification of plants by the number of the parts in question. Nothing could be more unnatural than to separate mosses for this reason from the other cryptogamic vegetables, and therefore Jussieu can scarcely believe these parts to be cotyledons; yet it is not possible to call them any thing else, and to suppose them a peculiar, and hitherto unheard of organ, would but increase the difficulty. Gærtner in the Introduction to his great work, p. 157, tells us he has seen many cotyledons in several *fuci* also, and that he suspects others of the more imperfect plants, hitherto referred to the *monocotyledones*, may be similarly circumstanced. It seems that too much, by far, has been taken for granted in this department, though the parts under consideration form the great hinge upon which all natural systems turn. It is only by analogy, that the great family,

Fuci.

family, or natural order, of *lichenes* has been judged monocotyledonous, an analogy which the *fuci*, if Gærtner be correct, render very doubtful. The germination of the *fungi* is at least equally uncertain.

Lichens.

Fungi.

I mean not however by any means to invalidate the importance of the distinction between such plants as have two or more cotyledons, and such as have only one or none, however inaccurate the terms commonly used to distinguish them may be. Much less am I inclined to throw any needless impediments in the way of those, who labour at the arduous and important study of natural classification, or to detract from the well-earned fame of such men as Gærtner and Jussieu, on account of difficulties and imperfections unavoidable in so abstruse a study. No real friend to truth and knowledge ever foments invidious rivalships in philosophy. The field of science is now so vast, that its different cultivators find the advantage of dividing their tasks, and thus the students of physiology, of natural systems, and of artificial ones, may all powerfully assist each other. Truth is pursued by different paths, and nothing is more pleasing than to see the various observers of Nature, in a Society like ours, mutually and harmoniously contributing, as we have all along done, to enrich the scientific hive. I would therefore conclude by recommending those, who have leisure and opportunity for the purpose, to observe for themselves the germination of the principal families of plants, not only of such genera as are in dispute, but of all about which there can be any doubt, most of which will easily be indicated by a comparison of Gærtner's work with the remarks in the foregoing pages.

Cultivators of science should divide their tasks, and assist each other without rivalship.

Norwich, *Nov.* 2, 1807.

XI. *Observations*

XI.

Observations on Nauclea Gambir, the Plant producing the Drug called Gutta Gambeer, with Characters of two other Species. By WILLIAM HUNTER, *Esq. Secretary to the Asiatic Society. Communicated by the President*.*

Gutta gambeer produced from a species of nauclea.

IT has been a question, among naturalists and writers on the materia medica, whether the little cakes or lozenges called *gutta gambeer* be prepared from the *mimosa catechu*, or the produce of a different plant †. This question, if not already determined, I am enabled to resolve by actual observation, having seen the substance made from a species of *nauclea*, of which I beg leave to offer the following description.

NAUCLEA GAMBIR.

Characters of the nauclea gambir.

Funis uncatus. Daun Gatta Gambir. *Rumph. Amb. t.* 5, 63, *t.* 34, *f.* 2.

Climbing. Branches round. Leaves ovate, pointed, Bracteas four, ovate, acute, spreading, very small, caducous.

Flowers aggregate, globular; composed of very numerous florets, crowded on a globular, naked, very small receptacle.

Cal. Perianthium common, none.

Proper, one-leafed, oblong, incrusting the germen, persistent; mouth five-cleft, divisions lanceolate, erect.

Cor. as in the Genus.

Stam. Filaments five, very short. Anthers oblong.

Capsule stalked, oblong, incrusted and crowned with the calyx; tapering to a point below; two-celled, two-valved; the valves adhering at the apex, splitting at the sides.

Seeds very numerous, oblong, very small, compressed, furnished at both ends with a membranous pappus.

The flowers, when fully spread, I suppose last a very short time; for although I have frequently looked for them, I was never able to find them, whence I have been obliged to omit the description of the pistil.

Method of preparing the gutta gambeer.

From the leaves of this shrub is prepared the substance called *gutta gambeer*, in two ways. The first is by boiling the leaves*. This process was performed under my inspection, by a Chinese, at *Prince of Wales's Island*. Seven catties (or 9⅓ lbs.) of the leaves, plucked clean from the stalks, were boiled in a large pot for one hour and a half, adding more water as the first wasted, till towards the end of the process, when it was inspissated to the consistence of a very thin sirup. When taken off the fire, and allowed to cool, it became solid. It was then cut into little squares, which were dried in the sun, turning them frequently. After one month, I weighed them, and found ten ounces and two drachms, troy weight.

Another mode of preparing it.

The *gambeer*, prepared according to this process, is of a brown colour; but from some parts of the *Malay* coast, and of *Sumatra*, it is brought in little round cakes almost perfectly

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* See Marsden's Sumatra, p. 243,—where he quotes, for a particular account of the manufacture, the second volume of the Transactions of the Batavian Society.

-fectly white. According to Dr. Campbell of *Bencoolen*, this is made by cutting small the leaves and young twigs, and infusing them in water for some hours, when a fæcula is deposited, which is inspissated by the heat of the sun, and moulded into round cakes.

Its qualities and use.

The *gambeer*, when first tasted, impresses on the palate a strong sensation of bitterness and astringency. But it afterward leaves a sweetish taste, which remains a long time. From these sensible qualities, it might reasonably be expected to prove useful in medicine. And accordingly, we are told that it has been found beneficial in angina and aphthæ, as well as in diarrhœa and dysentery. The drug was infused in water, to which it gave the colour of the infusion of bohea tea *. By the Malays it is mixed with lime, and applied externally to cuts, burns, boils, &c.

Chewed.

But the most frequent use of it is to chew, along with the leaves of betel, in the same manner as the *kut* (or catechu) in other parts of India.

Used in tanning and dyeing.

For this purpose the finest and whitest kind is selected. The red, being strong tasted and rank, is exported to *China* and *Batavia*, where it is used for the purposes of tanning and dyeing. For the first of these uses we might suppose, from its sensible qualities, that it is well calculated; and some rough experiments, which I have made on it with animal gluten, compared with those of Dr. Roxburgh on *kut*, evince it to be richer in *tannin* than that substance.

Rich in tannin.

Differences.

The chief places of manufacture are *Malacca*, *Siak* and *Rhio;* and the process of boiling is most generally practised; insomuch that the generality of manufacturers there are ignorant of there being any other. The colour and other qualities, they allege, depend on the vessel and the skill or attention of the operator. Thus an old manufacturer, with Chinese iron pots, will produce a whitish drug; whereas with a Malay iron pot its colour will be browner. The first cuttings also yield a whiter drug than the subsequent ones.

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* Murray, l. c. Seba. item. Buisson apud Degner. de Dysent. p. 207.

For

Culture.

For the cultivation of this plant a rich red soil is preferred. It gives the most luxuriant crop when the rains are frequent, but does not thrive in grounds that are apt to be flooded. On this account the side of a hill is esteemed better than any other situation.

Crops.

The plants are propagated from seed. In three months after sowing, they appear above the ground; after this they grow fast, and may be moved to the field when nine inches high. They are there planted at the distance of eight or nine feet, so that one *orlong* (of eighty yards square) contains about seven hundred plants. At the end of one year from the time when they are planted in the field, a small crop of the leaves is obtained. A larger is got in eighteen months; and the third at the end of two years, when the bushes have attained their full growth. They continue in their prime, and admit of being cut twice a year, during a period of twenty or thirty years, provided care be taken to keep the ground clean and the roots free from weeds. Their tops must be cut so as to prevent them from growing to a greater height than five or six feet.

Produce.

From good ground and a garden well kept, ten peculs (of 133$\frac{1}{3}$lbs. each) of dry gambeer are usually obtained on every orlong twice a year, or twenty peculs per annum. As it is cut every six months, and should then be boiled off, the leaves ought to be of the same age; but, from a want of means, it often happens, that the year is nearly expired before the cutting is done, which should have been made at the end of six months. In this case the young leaves yield a whiter drug than the old. As to the quantity afforded by each, in proportion to the weight of leaves, I have received contradictory information, so that I conclude little attention has been paid to this circumstance.

Price.

The price of the drug, at Prince of Wales's Island, varies from four to eight Spanish dollars per pecul. The finest and whitest kind is that formed into little round cakes or lozenges. It is sold by tale, at three dollars and a half for the *laxa* (or 10,000), and one *laxa* weighs about 40 catties. This gives 8$\frac{3}{4}$ dollars for a *pecul*.

Adulterated with sago.

The price of sago at Prince of Wales's Island is generally about three dollars per pecul. Hence the manufacturer is often tempted to adulterate his gambeer with this article, which mixes intimately, but may be detected by solution in water.

XII.

On the Variegation of Plants. In a Letter to Richard Anthony Salisbury, Esq. F.R.S. and L.S. by Thomas Andrew Knight, *Esq. F.R.S. and L.S.*[*]

My dear Sir,

Variegated plants not attended to by naturalists.

Though variegated plants have long occupied the care and attention of the gardener, it does not appear, that the peculiarities which distinguish them have much attracted the attention of the naturalist; and I am not acquainted with any experiments, which have been made either to discover the cause of variegation, or the effects produced by it. I am therefore induced to trouble you with an account of a few experiments, that I have made on one species of variegated plant, from which I obtained an unexpected and somewhat interesting result.

The variegated vine.

There is a kind of variegated vine, well known to gardeners, (the Aleppo), which affords variegated leaves and fruit; and as the grape, though small, possesses a very high flavour, and much richness, I wished to obtain some offspring either from its seeds or farina, with the hope of procuring berries of larger size, and at the same time of ascertaining whether its variegation would be transferred to the offspring.

Others impregnated with its farina became variegated.

With this object in view I extracted the immature stamina of the blossoms of the white chasselas, and white frontignac vines; and at the proper subsequent period I introduced the farina of the Aleppo vine: from this experiment I obtained, in the succeeding spring, many seedling plants. These plants, which were raised in a hot-bed, presented no singularity of character on their first appearance; but early in the succeeding summer I had the pleasure to observe purple stripes in the seed-leaves of several of them; and in the autumn the leaves of many were variegated. I did not however obtain a single plant, which promised to produce, or has subsequently afforded, either coloured fruit, or coloured leaves, free from variegation.

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[*] Trans. of the Linnean Soc. vol. IX, p. 268.

Farina of black grapes does not render white ones variegated.

When, on the contrary, I have introduced the farina of a black, or purple grape into the blossom of a white one, none of the plants I obtained have ever been variegated; and the colour of the leaves and fruit, which these in the first year afforded, indicated with certainty the colour of all the produce of such varieties, in whatever soil cuttings taken from them were subsequently planted.

In variegated vines the effect different.

But in the variegated vines the result has been wholly different; and though the leaves and fruit first produced by some of them contained more tingeing matter than any of the coloured kinds, they subsequently produced, even on the same tree, some bunches almost entirely black, others perfectly white, others lead-coloured with stripes of white, and others white with minute black stripes; and grapes of all the preceding colours are very frequently seen on the same cluster. The leaves are also subject to the same variations, and the colours in them are in some instances confined to the *upper*, in others to the *under* surface, and sometimes extend quite through; and both the leaves and fruit of some of the branches have become permanently colourless.

Their colouring matter combined in a peculiar way.

It appears therefore obvious, that the tingeing matter of variegated grapes, though probably not essentially different from that of others, is differently combined, and united to the plant; and as the variegated grape afforded offspring similar to itself, and none similar to other vines, which permanently afford coloured fruit, it may be confidently inferred, that the nature of the union between the tingeing matter and the plants is very essentially different.

The variegated plants hardy and vigorous, and therefore not owing to debility.

All the variegated plants, that I obtained from the farina of the Aleppo vine, are not only perfectly free from disease and debility of every kind, but many of them possess a more than ordinary degree of hardiness and vigour: and two of them appear much more capable of affording mature fruit, in the climate of England, than any now cultivated. It is therefore sufficiently evident, that the kind of variegation which I have described is neither the offspring off, nor connected with, disease or debility of any kind.

Variegated cabbage.

But the same inference must not be drawn respecting other variegated plants; for variegation itself appears to consist of several distinct kinds. The leaves of a variety of the common cabbage are often seen, in the cottage garden, curiously tinged with different shades of red and purple, like the leaves of the vines which I have described: but in the cabbage these colours combine and melt into each other, whereas in the vines the distinct colours are separated by well defined lines. The colours of the cabbage are transferred to its offspring, which is perfectly hardy and vigorous.

Spotted lettuce.

Leaves striped with white and yellow apparently connected with debility.

The spotted lettuce must also be classed with variegated plants, and the offspring of this is as hardy as those of other varieties: but the most common kind of variegation, in which the leaves are variously striped with white and yellow, though not the offspring, as some writers have imagined, of disease, is, however, closely connected with some degree of debility; possibly owing to the imperfect action of light on all such parts of the leaves as are either white or yellow. For I have observed, that variegated hollies are less patient of shade, than such as are wholly green; and I have never seen any plants, the leaves of which are wholly white or yellow, that continued to live beyond a single season. A variegated plant of the raspberry, which sprang from seed in my garden, became wholly white in the third year; but it perished in the succeeding winter, and I should be disposed to conclude, that plants the leaves of which are entirely white or yellow, cannot long survive; but that du Hamel* has described a variety of the peach tree, of which he says, "son bois, ses feuilles, ses fleurs, et son fruit, tant extérieurement qu'intériurement, sont tout à fait blancs." This variety is at present, I believe, wholly unknown to our gardeners; and I suspect, that it was always a debilitated plant, and that it in consequence exists no more. I am, &c.

Leaves wholly white or yellow he same.

THOMAS ANDREW KNIGHT.

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* In his Treatise on Trees.—*Article* Peach Tree.

XIII. *Method*

XIII.

Method of painting Linen Cloth with Oil Colours, so as to be more pliant, durable, and longer impervious to Water, than in the usual Mode. By Mr. WILLIAM ANDERSON, *of his Majesty's Dockyard, Portsmouth* *.

SIR,

I BEG leave to lay before the Society of Arts &c. the following improvements and observations, which I hope will be of service to the public.

Paint on canvas hardens and cracks it.

Having never heard or read of any method being discovered to prevent paint when laid on canvas from hardening to such a degree as to crack and eventually to break the canvas, and render it unserviceable in a short time; and having been an eye-witness for many years of much canvas perishing for want of such discovery in the immense quantities painted for covering seamen's hammocks, and for other uses on board his majesty's ships; I long had it under consideration to find out such an ingredient as, when mixed with paint, would preserve the canvas and paint laid thereon from the damages above mentioned, and after experiments for a considerable time, I have discovered such an article, and made trial of it with effect above three years.

Ingredient to prevent this discovered.

The canvas I have painted has been submitted to the inspection of the Navy Board, who are so perfectly satisfied with my new method, that general directions are now given to paint all canvas in his majesty's dock-yards in this manner; which, in addition to the advantages I have before mentioned, actually saves an expense of one guinea in every hundred square yards of canvas so painted, as I have fully stated to them. The ingredient I use is not only serviceable for ship's canvas, but also for canvas designed for paintings, for floor cloths, and for painted coverings within and without doors. I have no doubt of it being applied to many other purposes I am yet unacquainted with; as from

Saves expense.

Answers for paintings, floor cloths, &c.

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* Trans. of the Society of Arts, vol. xxvi, p. 136. The silver medal of the Society was voted to Mr. Anderson for this invention.

actual trials of near four years, I can vouch for its being a preservative to red, yellow, and black paints, when ground in oil and put in casks. When the paints were examined at the expiration of such time, they discovered no improper hardness; but when laid on the work with a brush, they dried in a remarkable manner, without addition of any of the usual drying articles. I still preserve some of these paints for future trials, and I believe this plan of preserving colours will be of essential use to colourmen, and other persons who purchase colours for exportation. The ingredient I use is perfectly simple, being a solution of yellow soap; and the composition for painting is made in the following manner:

Preserves paint in casks for use.

The composition.

To one pound of soap I add six pints of water in a vessel over the fire; in a few minutes after the boiling of the water the soap will dissolve; while hot it is to be mixed with oil paint, prepared as hereafter directed, and is then fit for immediate use. The above quantity of soap solution will be sufficient to mix with one hundred weight of paint. The first coat to be laid upon the canvas is to be intirely of this composition, without first wetting the canvas in the usual way. A very small proportion of it, or none, is necessary in the second coat; and the third coat should be of oil paint alone.

Old method of painting canvas for sea use.

The method heretofore practised in his majesty's dockyards for painting canvas, was as follows: The canvas was first wet with water, then primed with Spanish brown; a second coat given it of a chocolate colour, made from Spanish brown and black paint; and, lastly, finished with black. This mode is destructive, and more expensive than mine, in the proportion before mentioned.

New method.

In my method, to ninety-six pounds of English ochre ground in boiled oil, I add sixteen pounds of black paint, being one sixth in proportion of the ochre; this, when mixed, forms an indifferent black. The solution, made of one pound of soap and six pints of water, is to be added to this paint, and well united therewith; and without the canvas being previously wet, this composition is to be laid upon the canvas, as stiff as can conveniently be done with the brush, and this first coat will form a tolerably smooth surface. The second coat is to be formed of the same proportion of English ochre and black, without any soap solution; and the third or finishing coat, to be done with black paint as usual.

I am, Sir,

Your obedient humble Servant,

W. ANDERSON.

Master Painter of H. M. Dock Yard, at Portsmouth.

Portsea, Oct. 31st, 1806.

SIR,

AGREEABLY to the request in your letter, I have enclosed certificates relative to my new method of painting canvas; and I take the liberty of informing you of a method of obtaining from painted canvas, unserviceable, the whole of the colour laid thereon, and to do it at a very small expense. This I discovered since I last wrote to you, and I believe it will be of considerable advantage to government, who, for want of such a thought, have buried and burnt immense quantities of ships' hammock cloths, when found unserviceable, to prevent embezzlement from taking place. I suggested the idea to N. Diddems Esq. builder of Portsmouth yard, who communicated it to the honourable George Grey, Commissioner. I obtained leave to make an experiment, which I repeated thrice, and found that from one ton of painted canvas, unserviceable, I obtained, upon an average, four hundred weight of dry colour, in value to government nine pounds six shillings; the expense of the process not exceeding six shillings.

The colour may be obtained from old painted canvas,

at a trifling expense, by calcination.

This I effected by calcination, raking aside the ashes and sprinkling them with water, to prevent loss of paint through excess of heat. By passing the calcined matter through a fine sieve, it is perfectly prepared for grinding; it grinds well, possesses a good body for covering with, and dries well with a good gloss. Its increase of bulk, in comparison with common colour of equal weight, gives it the advantage of covering more work. The colours yielded by the calcination of different coloured canvas are as follows, *viz.* Canvas which has been painted with black paint only, produces a black colour. Canvas finished black, but which has had a previous red or yellow ground, will produce a dark chocolate colour. Canvas painted lead colour, will yield a good dark lead colour.

I am, Sir,

Your obedient humble Servant,

W. ANDERSON.

Testimonies to the superiority of canvas painted in the new mode,

Certificates, dated March, 1807, were received from the following persons, *viz.*

A. Stow, lieutenant and commander of the gun brig Steady, stating, that in the preceding month of October he had received on board his ship a set of hammock cloths, painted after the method invented by Mr. William Anderson, which had been constantly in use since the time above mentioned, and appeared fully to answer the end proposed, of rendering the canvas soft and pliable, and of preventing its cracking, or the paint peeling off, which in the old method had been a subject of much complaint.

John Pridy, lieutenant and commander of the Gladiator, and formerly commander of the Dapper, on which latter ship a set of hammock cloths, painted after Mr. Anderson's method, appeared fully to answer the end proposed.

P. F. Wyatt, oil and colourman, Portsea, stating, that he had seen canvas painted after Mr. Anderson's new method, which, after a trial of sixteen months, remained perfectly soft and pliable, the paint by no means cracking or peeling off, and that the gloss was retained, though it had been exposed to all weathers. He farther added, that he had seen the paint prepared by him from old painted canvas found unserviceable, and had worked and painted therewith; that it was, in his judgment, very good, and would answer either on canvas, wood, or iron.

and to the serviceableness of paint from old canvas.

Ns. Diddems, master shipwright, Portsmouth, dock-yard, stating that Mr. Anderson had proposed to him to obtain, by calcination, from old unserviceable painted canvas, the paint which had been laid thereon; that such experiment was made, and four hundred weight of dry serviceable paint prepared from one ton of such canvas; that he had seen it when ground in oil and laid on work, when it appeared to possess all the properties of good paint, and had therefore been recommended by him to the Navy Board.

SIR,

Samples sent to the Society.

IN answer to your letter of the 25th of April, in which you informed me, that the committee were desirous that I should furnish them with a sample of canvas painted in the old method, and another on my improved plan, I trust that I shall be able fully to comply with their requests. In the first place, I have sent a small sample of the residuum of the burnt canvas, fit for grinding in oil for paint, also a piece of canvas painted therewith, marked No. 1; another piece painted after the old method, marked No. 2; another piece painted according to my process, marked No. 3; and, lastly, a piece finished intirely with the new composition, marked No. 4; each sample having received three coats of paint. Upon examining No. 2, you will find it becoming from time to time more stubborn, in consequence of the paint hardening; and when a small ridge is formed in it, by pressing it between the finger and thumb, it will soon discover, that it is subject to crack, and by this means permitting the wet to enter it, will soon rot the canvas.

Time between the coats.

The space of time proper between laying on the new preparation and the second coat ought to be one entire day; but if saving time is an object, the second coat may be put on the day following the first; for if the canvas is placed in an advantageous situation for drying, the composition will dry or harden so as not to rub off.

The canvas does not stick together.

Canvas finished intirely with the composition, leaving it to dry one day between each coat, will not stick together if laid in quantities, as you will find by making experiments on the sample No. 4.

Requires little time to finish.

Since the Navy board have given directions for ships' canvas to be painted according to my new method, I find, upon calculation, that I have painted upwards of twenty thousand yards since N[illegible] a great part of which has not been hung up for painting and drying more than one week, as no more time could be allowed me, in consequence of ships sailing. My plan was therefore to lay on the composition the first day, to coat it the second day, and leaving one intermediate day, to finish it on the fourth. Three days were then allowed it to dry and harden, and when afterward taken down and folded together in cloths, containing sixty or seventy yards, they did not stick together.

Saving of three fourths of the labour in colour-grinding.

Having no means of giving information to persons concerned in grinding colours, so well as through the medium of the Society of Arts &c., I beg leave farther to relate how I have, for the last three years, saved the labour of three men out of four in grinding colours with the common mills employed for this purpose. One mill has ever been considered sufficient for a man to turn, whereas one man can now, with perfect ease, turn four mills; this is effected by placing two mills on each side of the winch, so close as only to leave room for the fly wheel to play between them. The spindles of each on either side are locked together by a small iron collar, with a pin passing through it. The distance of the mills thus paired from each, in order for the man's standing between them to turn, is two feet six inches. The distance of the arms of the winch screwed on the end of the spindles on either side is two feet two inches; the length of the arm is one foot six inches from the spindles to the bar across which the man clasps in order to turn.

Fly wheels.

Fly wheels at the extremity are impediments. Necessity was truly the mother of invention to me in this case, as I had great demand for paint, and I was not allowed men sufficient for the work in the common way.

Persons will scarcely believe, without seeing the experiment, the ease with which they turn. If a little extraordinary motion is first given them, and they are then left alone, they will continue to go round sixteen times; so that a man with one hand may turn them.

I am, Sir,

Your obedient humble Servant,

WM. ANDERSON.

SIR,

Testimony to the superiority of the painted canvas.

I HAVE stated to the Admiralty Board the several improvements made by me in paint work, and in consequence thereof they have desired the principal officers of our yard to report to them on their merits. The officers, who have for more than twelve months past daily had the execution of them under their inspection, have recommended the same in stronger terms, and the advantages thereof, to the lords commissioners, beyond my statement. I have inclosed to you a certificate relative to the ship Hibernia, which arrived here the 12th of May last, and for which vessel I painted a set of hammock cloths, containing thirteen hundred yards of canvas, in June 1806, after my new method.

I am, Sir,
Your obedient humble Servant,
WM. ANDERSON.

Portsmouth, Nov. 27, 1807.

The foregoing letter was accompanied with a certificate from Captain Hicks, and William Trounsell, carpenter of his majesty's ship Hibernia, stating, that the hammock cloths on board the said vessel were painted in June 1806, after Mr. William Anderson's method; that they were pliable, and did not crack, nor the paint peel off, and were, in their opinion, preferable to those painted in the common way.

SIR,

I BEG leave to trouble you with a farther certificate relative to my method of painting canvas.

I have also discovered a lead coloured paint, highly advantageous for all iron-work exposed to the weather, and preferable to that commonly made from white lead and black. The preparation is as follows:

Process for Lead Coloured Paint on Iron.

Lead coloured paint for iron.

I take a fire shovel, and put a small quantity of common litharge thereon, and place it over the fire. I then take a small portion of flour of brimstone between my fingers, and scatter it over the litharge, when the same is sufficiently

Your hum

Durability of the painted canvas.

This letter was accompanied wit J. B. Harrison, lieutenant and comm brig Red-Breast, dated Feb. 26, 18 hammock cloths on board the said v Mr. Anderson, in April 1807, and been constantly in wear, and exposed and night; that owing to this new preserved supple, without cracking, hered to them in a far superior manne of painting.

XIV.

Account of the Royal Botanical Gar *St. Vincent.* *By* Dr. Alexander

SIR,

State of the botanical

FROM my long silence you will cc lectful or ungrateful to the Society, b The reason is, I had nothing of coi

I am grieved to inform you, that I have lost one of my nutmeg trees; unfortunately the other, which prospers luxuriantly, turns out to be a male plant, consequently worth nothing. I blame myself in some measure, for this loss, by taking too much care of it, and not letting nature take her own way. Unluckily the war precludes any correspondence with Cayenne, or I would have replaced it from thence. The same cause has cut off all supplies from other parts. Through the medium of a gentleman who was here last year from Cuba, I expected to have had, before now, some of the productions of Mexico and adjacent parts of the continent, particularly myroxylon, or balsam of Peru; however, if I do not procure it through that channel, I have found out another from whence I have hopes.

Gomertur palm.

The Gomertur palm, which produces the material for cordage in the East Indies, is thriving here surprisingly, and, I think, might be rendered a valuable production to these islands. The mode of its producing the fibrous web, and the guard or protection surrounding, clearly point out, that nature intended it for the use of man; one tree produces an astonishing quantity. I think the fibres from the plants in this garden are stronger than the specimens I have seen from the East Indies. A small piece of the web, with its protector, I now transmit you. I have great reason to think that but few plants have been raised by the planters in the different islands, from the large quantities of seeds I have dispersed amongst them. The fact is, that no attention, except by a few individuals, is paid to any other plant but the sugar cane, and no other is in estimation with them.

Only the sugar cane attended to by the planters.

The bread-fruit, although one of the most valuable productions yet sent them, is neglected and despised, unless by a few persons. They say that negroes do not like it, and will not eat it, if they can get any thing else; but this is not really the case, as I know, and can declare from experience, that the very reverse is the fact, when once they are a little accustomed to it. The fact is, that the planters hate giving it a place on their estates, as they regard it as an intruder on their cane land, and they dislike any other object

They even dislike the bread fruit tree,

object but canes. As to futurity, they think nothing of what may be the wants of themselves or negroes three or four years hence. Even their most valuable mill-timber, than which nothing is more daily-wanted by them, they are constantly destroying instead of preserving. They import it at an exorbitant rate, and the importation is precarious. With proper economy and management, there are few necessaries for themselves or negroes, but which might be raised on their own estates, instead of importing them from America, unless it be lumber, and, probably even that might be done in time in the back, cool, and mountainous situations. I am trying what may be done from the pine tribe. and are in every respect negligent of future benefit.

Cinnamon. I am happy that many are now paying some attention to the cinnamon, as the demands on me for the plants are frequent, which I impute to the specimens of it which I have shown.

Black pepper. The black-pepper plants have not yet produced; I have them in plenty, and am trying them in various situations, and can easily increase them by cuttings; unluckily I can procure no information as to their culture in the East Indies, or of the soil or situation in which they thrive best.

Cloves. I send you some more cloves, the last year's produce of two small trees; next year I expect from several others: you will also find inclosed a lump of gum resin from cuchola odorata. As it issues in large quantities from wounds in the bark, it might be procured in plenty from Trinidad, if found useful. Trees of it, of enormous size, are abundant there. Other specimens of terra japonica would have been sent with some other articles, if all my attention had not been engrossed about the late addition to the garden: the same cause has prevented me from excursions to other islands for larger supplies of plants. I remain, with most sincere regard.

Sir,

Your obliged and obedient Servant,

ALEXANDER ANDERSON.

SIR,

SINCE your last letter to me very little matter interesting to the Society has occurred, and few acquisitions made to the garden subservient to medicine or commerce. War interrupts correspondence in natural history as much as speculations in commerce.

Correspondence interrupted by the war.

For 18 months past I have had expectations of some useful plants from Mexico, and other Spanish colonies in that quarter, by the way of Cuba, but thence the transportation must be circuitous by North America, and after that subjected to loss and interruption, before they can reach St. Vincent. I have therefore given up all hopes while the war continues.

As the Society may be desirous to know the present state of the garden, I have transmitted a catalogue of the variety of plants it contained on the 24th of September last: there are many more from different quarters received without names, or those that are known by the aborigines, and I cannot arrange them until they flower. I am, with great respect,

Catalogue of plants.

SIR,
Your most obedient humble Servant,
ALEXANDER ANDERSON.

The catalogue alluded to, which is dated September 24, 1806, enumerates 67 commercial and medicinal plants; 49 esculent; 101 medicinal; and 63 economical: 76 valuable woods; 88 fruits; and 929 curious or ornamental exotics. There are likewise many others, which, not having flowered in the garden, cannot be ascertained.

XV.

Query on Accidents frequently happening to Dies with which Medals are struck. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR.

READING the life of Rambert Dumarest in a French periodical publication, I was struck with the following remark.

"To

Dies of medals very liable to be broken.

"To the specimens of Mr. Dumarest's talents I ought to add the medal on the peace of Amiens, the execution of which was awarded to him after a public competition. Unhappily, we are informed, one of the dies was crushed under the mill. This is a very frequent accident, and discouraging to the artist. Dumarest experienced it in a very vexatious manner; he was obliged to make almost every die he executed over again, and for one of his medals this happened no less than eight times following. The causes of it deserve inquiry. No doubt they depend on the quality of the steel, the care with which it has been forged, its temper, and the influence of the atmosphere; but still more on the mills, and the care taken in coining."

Are there any means of preventing this?

As I cannot gain any instructions on this point from the books I have at hand, and have no opportunity of acquiring information from any artist, I should be happy to be informed through the medium of your Journal, whether there be not means of preventing this.

It appears, that Dumarest was in England some time, and employed in the manufactory at Soho by Mr. Bolton, so that he must probably have been acquainted with many precautions taken there against an accident, to which it might be supposed the dies would be particularly liable under the pressure of Mr. Bolton's powerful machinery.

I am, Sir,

Your very obedient humble Servant.

C. O. T.

SCIENTIFIC NEWS.

French translation of Archimedes.

IT is somewhat remarkable, that the works of a mathematician so justly celebrated as Archimedes has never been translated into any modern language till lately. I believe his Arenarius is the only tract we have of his in English; but I am informed a French translation of the whole of his works, that have reached us, has lately been published at Paris.

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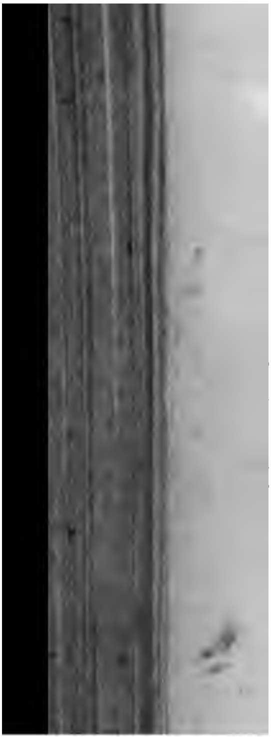

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